

To: Allegra LeGrande <legrande@ldeo.columbia.edu>
Subject: [Fwd: St. Elias ice cores]
Date: 01/09/07 13:46:26

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> From: Cameron Wake <cameron.wake@unh.edu>
> To: gschmidt@giss.nasa.gov
> Cc: Karl Kreutz <karl.kreutz@maine.edu>
> Subject: St. Elias ice cores
> Date: 28 Dec 2006 15:57:07 -0500

> Hi Gavin,

> I saw your talk at AGU and wanted to follow up with you. I was
> impressed by the fact you have added stable isotopic tracers to your
> model in an effort to recreate the ice core signals. I see this as a
> wonderful step forward to get the paleo community talking more with
> the modeling community.

> I am wondering if you might be interested in helping us better
> understand our stable isotope records from two ice cores from the St.
> Elias range - one from 3000 m and one from 5400 m, and a sediment
> record from a nearby lake (800 m). The two ice cores show different
> stable isotope records (no surprise there based on previous work),
> but especially with respect to a "shift" at about 1840 AD, that we
> think represents a change from primarily zonal flow (before 1840) to
> mixed zonal and meridional flow after 1840. Our results are
> summarized in the attached paper by Fisher et al (which includes some
> very basic stable isotope modeling).

> My question: Are you interested in comparing your model output with
> our stable isotope records for two different levels in the atmosphere
> and perhaps help us understand what is causing the shift?

> Regards,

> Cameron

> PS - I have been following RealClimate for years (originally brought
> to my attention by Eric Steig) and want to congratulate you on a
> superb effort.

> [OBJ]

> Cameron P. Wake, PhD
> Research Associate Professor
> Climate Change Research Center
> Institute for the Study of Earth, Oceans, and Space (EOS)
> Morse Hall, University of New Hampshire
> Durham, NH 03824
> tel: 603-862-2329; fax: 603-862-2124
> web: <http://www.ccrcc.sr.unh.edu/~cpw>

PDF document attachment (Fisher_2006_GeogPhysQuat.pdf)

STABLE ISOTOPE RECORDS FROM MOUNT LOGAN, ECLIPSE ICE CORES AND NEARBY JELLYBEAN LAKE. WATER CYCLE OF THE NORTH PACIFIC OVER 2000 YEARS AND OVER FIVE VERTICAL KILOMETRES: SUDDEN SHIFTS AND TROPICAL CONNECTIONS

D.A. FISHER*, C. WAKE, K. KREUTZ, K. YALCIN, E. STEIG, P. MAYEWSKI, L. ANDERSON, J. ZHENG, S. RUPPER, C. ZDANOWICZ, M. DEMUTH, M. WASZKIEWICZ, D. DAHL-JENSEN, K. GOTO-AZUMA, J.B. BOURGEOIS, R.M. KOERNER, J. SEKERKA, E. OSTERBERG, M.B. ABBOTT, B.P. FINNEY and S.J. BURNS; first, eighth, tenth, eleventh, fifteenth, sixteenth and seventeenth authors : Geological Survey of Canada, 601 Booth Street, Ottawa, Ontario K1A 0E8; second and fourth authors : Climate Change Research Center, Morse Hall, University of New Hampshire, Durham, New Hampshire 03824, United States; third, sixth and eighteenth authors : Climate Change Institute and Department of Earth Sciences, University of Maine, Orono, Maine 04469, United States; fifth and ninth authors : Quaternary Research Center, 19 Johnson Hall, Box 1360, University of Washington, Seattle, Washington 98195, United States; seventh and twenty-first authors : Department of Geosciences, University of Massachusetts-Amherst, Amherst, Massachusetts 01003, United States; twelfth author : 9553 77th Avenue, Edmonton, Alberta T6C 0M3; thirteenth author : Niels Bohr Institute, Juliane Maries Vej 30, University of Copenhagen, DK-2100, Copenhagen East, Denmark; fourteenth author : National Institute of Polar Research, Tokyo 173-8515, Japan; nineteenth author : Department of Geology and Planetary Science, University of Pittsburgh; Pittsburgh, Pennsylvania 15260; United States; twentieth author : Institute of Marine Sciences, University of Alaska Fairbanks, Fairbanks, Alaska 99775, United States.

ABSTRACT Three ice cores recovered on or near Mount Logan, together with a nearby lake record (Jellybean Lake), cover variously 500 to 30 000 years. This suite of records offers a unique view of the lapse rate in stable isotopes from the lower to upper troposphere. The region is climatologically important, being beside the Cordilleran pinning-point of the Rossby Wave system and the Aleutian Low. Comparison of stable isotope series over the last 2000 years and model simulations suggest sudden and persistent shifts between modern (mixed) and zonal flow regimes of water vapour transport to the Pacific Northwest. The last such shift was in A.D. 1840. Model simulations for modern and "pure" zonal flow suggest that these shifts are consistent regime changes between these flow types, with predominantly zonal flow prior to ca. A.D. 1840 and modern thereafter. The 5.4 and 0.8 km asl records show a shift at A.D. 1840 and another at A.D. 800. It is speculated that the A.D. 1840 regime shift coincided with the end of the Little Ice Age and the A.D. 800 shift with the beginning of the European Medieval Warm Period. The shifts are very abrupt, taking only a few years at most.

RÉSUMÉ Comportement des isotopes stables dans les carottes de glace des monts Logan et Eclipse et les sédiments lacustres du lac Jellybean. Le cycle de l'eau dans le Pacifique nord sur 2000 ans et sur cinq kilomètres verticaux : changements brusques et connexions tropicales. Trois carottes de glace prélevées à proximité du mont Logan, combinées à une coupe stratigraphique du lac Jellybean, couvrent une période comprise entre 500 et 30 000 ans. Elles renseignent sur les taux de changement de la composition isotopique de la troposphère. La région étudiée est importante au niveau climatologique puisqu'elle est au point de convergence des ondes de Rossby et de la dépression des Aléoutiennes. La comparaison entre la composition isotopique depuis 2000 ans et les résultats des simulations suggère des changements brusques et persistants entre les régimes de transport de vapeur d'eau modernes et zonaux dans le nord-est du Pacifique, où le dernier changement s'est produit en 1840 de notre ère. Les simulations indiquent que les changements de flux correspondent aux changements de régime, avec un flux zonal avant ca 1840 pour passer au type moderne ensuite. Les forages à 5,4 et 0,8 km d'altitude montrent un changement en A.D. 1840 et un autre en l'an 800. On présume que ces changements de régime coïncident respectivement avec la fin du Petit Âge Glaciaire et le début de la période médiévale chaude, ces changements s'étant produits en quelques années seulement.

INTRODUCTION

In Holocene times outside the tropics, stable isotopes of water given in terms of oxygen [$\delta(^{18}\text{O})$] and deuterium [$\delta(\text{D})$] are usually correlated geographically and temporally to the temperature of the site (Dansgaard *et al.*, 1973; Jouzel *et al.*, 1984). The standard unitless measure of ratio, d , is:

$$\delta(\text{D}) = 1000 * (([\text{D}]/[\text{H}])_{\text{sample}} - ([\text{D}]/[\text{H}]_{\text{SMOW}}) / ([\text{D}]/[\text{H}]_{\text{SMOW}})$$

where SMOW denotes "standard mean ocean water". Stable isotopic ratios are also affected by the water cycle history between the sources and the site and a generous amount of stratigraphic local noise (Fisher, 1992; Fisher *et al.*, 1996). The deuterium excess, d , is a derived variable ($d = \delta(\text{D}) - 8 * \delta(^{18}\text{O})$) that is a conserved quantity for each source region (Merlivat and Jouzel, 1979; Johnsen *et al.*, 1989). Although it is also sensitive to water cycle history that bears the moisture to the sites (Fisher, 1991; Fisher *et al.*, 1996), d is an indicator of the source ocean temperature. The geographic distribution of $\delta(^{18}\text{O})$ and $\delta(\text{D})$ has been modeled with the help of GCMs (Jouzel *et al.*, 1997) and intermediate complexity models (Fisher, 1990, 1992; Kavanaugh and Cuffey, 2003). The latter are used to interpret the results presented here.

Previous work on the 300-years-long Mount Logan $\delta(^{18}\text{O})$ series (Holdsworth *et al.*, 1992; Fisher, 2002) demonstrated that it is out of phase with other proxy temperature series, showing a Little Ice Age with "warmer," more positive, values. At 5 400 m asl, $\delta(^{18}\text{O})$ is not a temperature indicator, and a goal of this paper is to explain what it does indicate.

Three ice cores on and near Mount Logan were obtained by a Geological Survey of Canada (GSC)-led consortium of American, Japanese and Canadian groups. The stable isotopes of two of these cores and the isotopic record from Jellybean Lake just south of Whitehorse are compared. The specifics of the various core sites are given in Table I and Figure 1.

PROSPECTOR RUSSELL COL, PRCOL ICE CORE

The top of the Mount Logan massif is a 5 400 m asl plateau about 10 x 25 km dotted with several ~700-metre-high cones, the tallest of which is Mount Logan (Fig. 1B). Between the cones are relatively flat areas with low ice velocities. Figures 1C and D show the topography of the PRCOL drill site

and the 300-years NWCol ice core obtained in the early 1980s (Holdsworth *et al.*, 1992). The surface topography and velocities suggest that the PRCOL drill site is presently close to a centre of flow. Given the low annual temperature and lack of uncovered area for ice to expand into, this situation has probably been stable over the Holocene. Older ice is within the bottom-most 5 % of the thickness and possibly affected by flow discontinuities. Here, however, we are focusing on the isotope record from only the last 2 ka, which are well away from the bed. Solid electrical conductivity records (ECM) correlate with volcanic acid horizons (Hammer, 1983). Figure 2 shows a comparison between Eclipse and PRCOL records. The PRCOL record is placed on a time scale using a model, recent accumulation rates and the major ECM (acid) peaks of Katmai, Laki, a large unknown peak at A.D. 1516 and White River (Clausen *et al.*, 1995; Clague *et al.*, 1995; Zheng *et al.*, 1998; Yalcin and Wake, 2003). The unidentified A.D. 1516 event must be "local" because, although it is the largest peak in the last 550 years, it has no prominence in the Eastern Arctic (Clausen *et al.*, 1995; Zheng *et al.*, 1998). Between the fixed points of this time scale, the uncertainty could be ± 3 years. The White River candidate ECM peak stands out and is within a metre of where it is expected. Thus, it is assigned the age of the most recent large eruption (A.D. 803) that deposited the white ash layer over most of southern Yukon (Clague, 1995). Using the time scale for PRCOL described in the text, the six largest PRCOL ECM peaks coincide with six large sulfate peaks of the Eclipse core over the 530 years. Presently, ages beyond A.D. 803 are only pinned by a very clear transition into ice-age ice with a sudden reduction in ECM coinciding with a large drop in $d(^{18}\text{O})$ (Wolfe *et al.*, 1997). This transition is assigned the Greenland date of 11 550 cal BP (Johnsen *et al.*, 1997).

By scanning 100-years segments of the Eclipse sulfate series across the PRCOL ECM time series, correlation coefficients between 0.22 and 0.33 were found always with a relative lag time of ≤ 2 years. Allowing for these series' low autocovariance, the correlations are significant at the 95 % confidence level. The time lag was close to 0 near the three pinning points. The only century in which the correlation was not significant at 95 % was A.D. 1770-1670, when the peaks are in phase but larger in PRCOL. This difference could be due to noise, or because these peaks are from more distant sites and show up more plainly at the higher site. Peaks numbered

TABLE I

Specificities of core sites

Site	Elevation (m asl)	Latitude	Longitude	Mean temperature (°C)	Main Institutions	Maximum age (BP)	Ice accumulation rate (m/yr)	Depth reached (m)
PRCOL	5340	60.59	140.50	-29	GSC, UMaine	30 000	~0.65	188 (bed)
Eclipse	3017	60.51	139.47	-5	UNH, UMaine	~1000	1.38	345
King Col	4135	60.58	140.60	-17	NIPR	~300	~1.00	220.5
Jelly Bean Lake (1650)	800	60.35	134.80	-1	Umass, UPitt, UAF	~7500	n/a	n/a

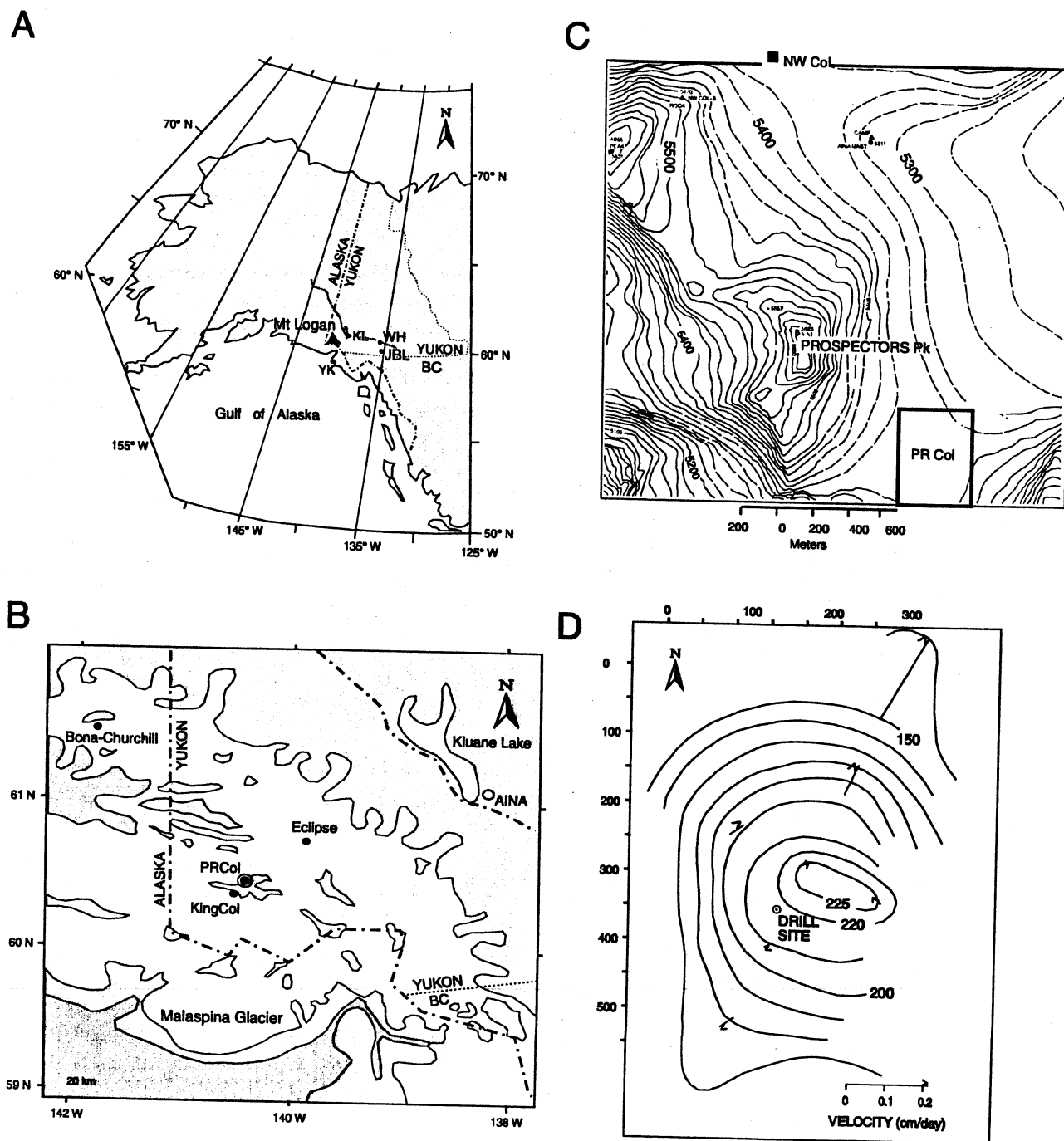


FIGURE 1. Study site locations. (A) Location of Mount Logan, Jellybean Lake (JBL), Whitehorse (WH) and Kluane Lake (KL). (B) Location of the ice core sites. (C) Surface topography for the PRCol and the older NWCol drill sites with contours in metres above sea level. The PRCol drill site is close to the centre of the rectangle at the bottom. The surface here is flat and horizontal. The PRCol weather station is close to the drill site. (D) Ice thickness (metres) and horizontal velocity map. The drill site is close to the zero-velocity point of this map.

Sites d'étude. (A) Localisation du mont Logan, du lac Jellybean (JBL), de Whitehorse (WH) et du lac Kluane (KL). (B) Localisation des sites de forage. (C) Topographie des sites PRCOL et NWCol. Le site de forage de PRCOL se situe au centre du rectangle. Cette surface est plane et à l'horizontale. La station météorologique de PRCOL se situe à proximité du site de forage. (D) Épaisseur de la glace (en mètres) et carte de vitesse.

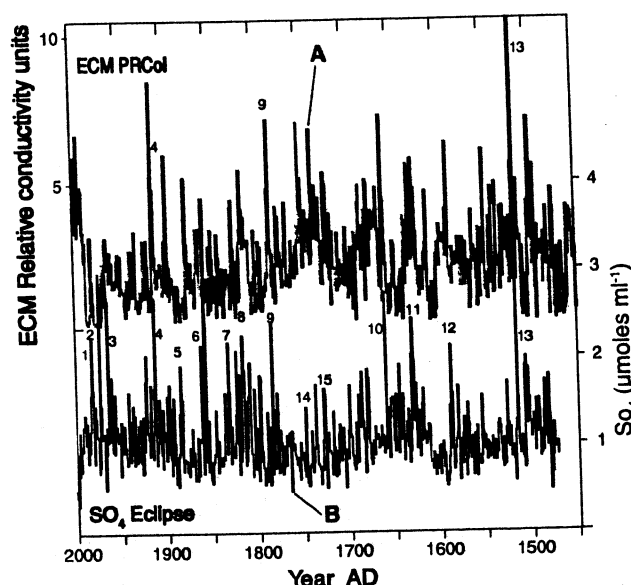


FIGURE 2. A 530-years ECM record for the PRCol core compared to the sulfate concentration from the Eclipse core. The ECM record mainly reflects the acidity of the core whose peaks are mostly volcanically derived. The sulfate is also a volcanic eruption marker, although some volcanic events give off large amounts of HCl. For example, Katmai (A.D. 1912) is a very strong chloride event in the St. Elias Mountains. The A.D. 1516 unknown event is the largest peak recorded in the Eclipse and PRCol. Peaks numbered 4, 9 and 13 (Katmai, Laki and unknown) were used to establish the PRCol time scale. Named peaks in the Eclipse core are: (1) El Chichon, (2) Tiatia, (3) Sheveluch, (4) Katmai, (5) Krakatoa, (6) Fuego, (7) Babuyan, (8) Tambora, (9) Laki, (10) Katla, (11) Fumas-Vesuvius, (12) Billy Mitchell, (13) unknown A.D. 1516.

La série temporelle de conductivité électrique du forage de PRCol est comparée à la concentration en sulfate du forage d'Eclipse sur 530 années. Cette série est le reflet de l'acidité, où les valeurs maximales sont associées à l'activité volcanique. Le sulfate est un marqueur des éruptions volcaniques, et plusieurs événements ont produit de fortes quantités de HCl. Par exemple, l'éruption de Katmai (A.D. 1912) a produit une quantité importante de chlore sur le mont St-Elias. L'éruption inconnue de l'an 1516 constitue l'événement le plus important pour les forages d'Eclipse et de PRCol. Les maximums 4, 9 et 13 (Katmai, Laki et inconnu) sont utilisés pour établir l'échelle temporelle de PRCol. Les pics du forage d'Eclipse sont les suivants : (1) El Chichon, (2) Tiatia, (3) Sheveluch, (4) Katmai, (5) Krakatoa, (6) Fuego, (7) Babuyan, (8) Tambora, (9) Laki, (10) Katla, (11) Fumas-Vesuvius, (12) Billy Mitchell, (13) inconnu A.D. 1516.

4, 9 and 13 in the PRCol record are major sulfuric acid peaks and 4 (Katmai) is high in both sulfuric and hydrochloric acid, as it is uniquely in the Eclipse record.

ECLIPSE ICEFIELD

Two cores (345 and 130 m) were recovered in 2002 (Table I). The presence of discrete ice layers in the Eclipse ice core accounting for 5% of the net accumulation by weight demonstrates that a limited amount of surface melting occurs at the Eclipse site during summer. Meltwater percolation does not significantly alter the glaciochemical records of the Eclipse ice core, as evidenced by the preservation of clear seasonal

signals in the major ion and oxygen isotope records. This allows dating of the cores via multi-parameter annual layer counting.

Age control on the chronology established via annual layer counting is provided by the A.D. 1963 and A.D. 1961 ^{137}Cs reference horizons as well as volcanic reference horizons (Katmai, 1912; Tambora, 1815; Laki, 1783; Kuwae, 1453) developed through statistical analysis of the high-resolution sulfate record. In some cases, these identifications have been independently verified using tephrochronology.

The cores (except for the top 60 metres of the 345 m core that was drilled with a titanium barrel for trace element analyses) were sampled continuously at high resolution for major ions and stable isotopes to establish a detailed chronology. Sample resolution ranged from 6 to 15 cm for major ions and 2 to 15 cm for stable isotopes. Stringent core processing techniques were used to ensure samples were free of contamination at the ng g^{-1} level. Blanks prepared on a frequent basis showed no contamination of samples during processing of the core. Samples were analyzed for major ions (Na^+ , NH_4^+ , K^+ , Mg^{2+} , Ca^{2+} , Cl^- , NO_3^- , SO_4^{2-} , $\text{C}_2\text{O}_4^{2-}$) via ion chromatography using a 0.5 ml sample loop in a dedicated laboratory at the University of New Hampshire Climate Change Research Center. The cation system used a CS12A column with CSRS-ultra suppressor in auto suppression recycle mode with 20 mM MSA eluent. The anion system used an AS11 column with a CSRS-ultra suppressor in auto suppression recycle mode with 6 mM NaOH eluent. Oxalate was not quantified in the 1996 core. Stable isotopes (d^{18}O and $\text{d}(\text{D})$) were analyzed at the University of Maine Stable Isotope Laboratory with an Autoprep CO_2 equilibration system coupled to a VG SIRA instrument. A section of each core was analyzed for radionuclides (^{137}Cs) via gamma spectroscopy.

Annual layers were identified by summer-to-winter variations in the oxygen isotope ratio and sodium concentrations. The annual cycle of maxima in summer precipitation and d^{18}O minima in winter precipitation observed at Eclipse and other ice core sites is related, at least in part, to the temperature at which evaporation and condensation occur (Fisher, 1996). The annual cycle of sodium concentration maxima in winter and minima in summer is related to pronounced seasonal changes in the influx of marine aerosols (Whitlow *et al.*, 1992). Increased storminess and higher wind speeds in the Gulf of Alaska during winter result in enhanced entrainment of sea salt aerosols and more frequent advection of marine air masses into the St. Elias Mountains in winter, producing the winter peaks in sodium concentrations.

JELLYBEAN LAKE

Jellybean Lake (JBL) is a small (0.4 km^2), relatively deep (11.6 m) lake located 800 m asl. The lake basin is groundwater-fed from a confined aquifer recharged 12 km to the east on a broad ridge $\sim 1640 \text{ m}$ asl. Inflow and outflow are sub-surface. The water column is thermally unstratified and chemically mixed. International Atomic Energy Agency precipitation data from Mayo and Whitehorse form a local meteoric water line and define the isotope values for unmodified mean annual

precipitation. The value, -21‰ SMOW, corresponds with local spring water and Jellybean Lake water. Although other lake data in the southern Yukon indicate a range of $\delta(^{18}\text{O})$ -variability due to evaporation of up to 10‰ , water residence times in Jellybean Lake are apparently too short for evaporation to modify the lake-water $\delta(^{18}\text{O})$. The $\delta(^{18}\text{O})$ of the Jellybean Lake surface sediment calcium carbonate was -19.9‰ VPDB, and demonstrates isotopic equilibrium between calcite and water. Thus, changes in sedimentary carbonate $\delta(^{18}\text{O})$ are inferred to reflect changes in lake-water $\delta(^{18}\text{O})$ that, in turn, reflect changes in input-water $\delta(^{18}\text{O})$. The latter is controlled by regional climate (Anderson, 2004).

Sediment cores were retrieved from the deepest part of Jellybean Lake and continuously sampled at high resolution for oxygen and carbon isotopes. Lake-water calcium concentrations are sufficient to sustain bioinduced calcification such that carbonate sedimentation occurs at all water depths, and core sediments are nearly pure authigenic micrite. Samples were freeze dried, examined for purity and powdered before sub-sampling, CO_2 extraction and isotope mass spectrometry.

The Jellybean Lake calcite $\delta(^{18}\text{O})$ is on the PDB scale, which is very close to the $\delta(^{18}\text{O})$ of the ambient lake water on the SMOW scale (Anderson, 2004). Our method has taken the difference between calcite $\delta(^{18}\text{O})$ shifts in PDB over certain time intervals and compared them with the differences between water in VSMOW. The conversion from VPDB to VSMOW for calcite in lake water is temperature dependent. The temperature fractionation factor is small (-0.25‰ per $^{\circ}\text{C}$). Thus, if the temperature at A.D. 1840 changed by $2\text{ }^{\circ}\text{C}$, the per mil values could have changed by $\pm 0.5\text{‰}$. So the "temperature-error" in the A.D. 1840 Jellybean Lake shift of 1.5‰ is, at most, $\pm 0.5\text{‰}$.

The sediment core chronology is based on ^{210}Pb , ^{137}Cs , seven AMS ^{14}C measurements on identifiable macrofossils and the White River tephra (Clague *et al.*, 1995). A linear interpolation between dated depths was used to determine ages of sediment samples. The chronology indicates a fairly uniform sedimentation rate of about 0.05 to 0.075 mm a^{-1} . Based on sedimentation rates and sample thickness, the oxygen and carbon isotope samples integrate 3 to 6 years in the uppermost 16.5 cm , and 10 to 30 years for the remainder of the core that spans to about 7500 cal BP (Anderson, 2004; Anderson *et al.*, 2005).

RESULTS

SUDDEN SHIFTS IN THE MID-1800S AND A.D. 800

Figure 3A presents the last 530 years of $\delta(\text{D})$ and $\delta(^{18}\text{O})$ for Eclipse and PRCol cores, respectively, and Figure 3B shows the PRCol deuterium excess, d . In A.D. 1840 ± 3 years, there is a 3.5‰ shift in $\delta(^{18}\text{O})$ of PRCol but no shift in $\delta(\text{D})$ of Eclipse. There is, however, a significant decrease in the Eclipse accumulation rate between A.D. 1841–1861 (Yalcin *et al.*, 2004). At the PRCol site $\delta(^{18}\text{O})$ and d are in anti-phase (Fig. 3A–B). The A.D. 1840 shift in d ranges from pre-A.D. 1840 values of $\sim 15\text{‰}$ to post values of $\sim 19\text{‰}$. There is also a similar marked shift in $\delta(^{18}\text{O})$ in the NWCol core (Holdsworth *et al.*, 1992), which also marks the beginning of an increase in accumulation rates (Moore *et al.*, 2002).

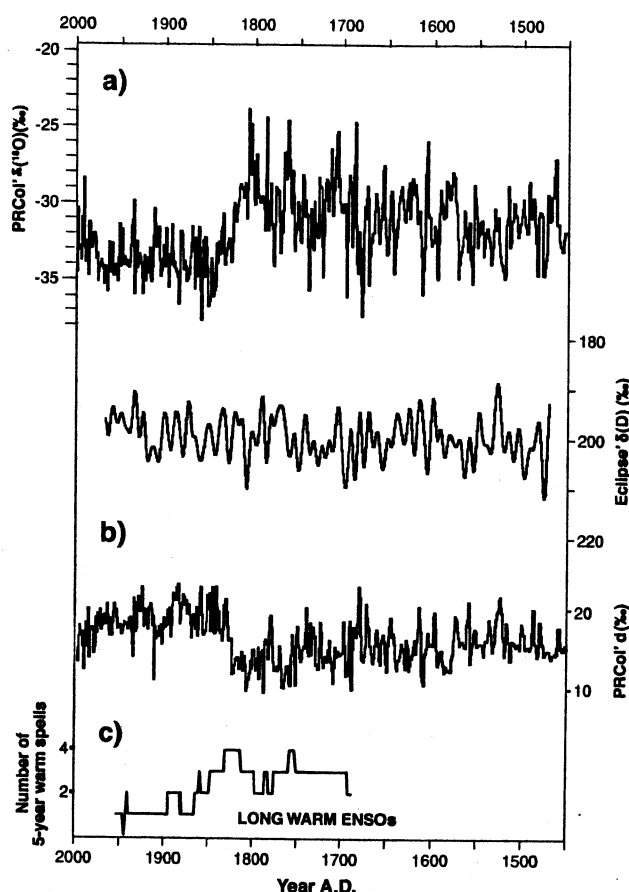


FIGURE 3. (A) The $\delta(^{18}\text{O})$ for PRCol (5 340 m asl) and the $\delta(\text{D})$ for Eclipse (3 017 m asl) ice core sites, smoothed with a 5-years low pass filter. At PRCol there is an abrupt shift in $\delta(^{18}\text{O})$ of about 3‰ ca. A.D. 1840, that is not evident in the Eclipse record. The older NWCol Logan core also has a similar shift at the same date. We suggest that prior to A.D. 1840 the moisture flow was predominantly zonal with North Pacific sources of water, and after A.D. 1840 the flow was mostly "modern" delivering moisture from more southerly sources. The higher site receives relatively much more distant southern warm-source moisture than the lower. Compare the A.D. 1840 shift to that of A.D. 1976. (B) The deuterium excess plot for PRCol, indicating a major shift of moisture source ca. A.D. 1840. The larger excess points to warmer source oceans providing the moisture. (C) A plot of ENSO strength statistics implying that a regime shift occurred in the mid-19th century. The synoptic situation that would go along with the shift is that a deeper more northwest-centred Aleutian Low would draw moisture from farther south.

(A) Séries temporelles de $\delta(^{18}\text{O})$ et $\delta(\text{D})$ lissées sur 5 ans avec un filtre passe-bas pour les forages de PRCol (5 340 m asl) et d'Eclipse (3 017 m asl). On observe un changement brusque d'environ 3‰ de $\delta(^{18}\text{O})$ en A.D. 1840 dans le forage de PRCol, qui ne s'observe pas dans le forage d'Eclipse. Le forage de NWCol montre un comportement similaire à celui de PRCol. On pense que le flux d'humidité est zonal en A.D. 1840, avec des sources provenant du Pacifique nord tandis qu'après A.D. 1840, il est alimenté par des sources plus au sud. Le plus haut site reçoit plus d'humidité d'une source chaude méridionale que le site le plus bas. (B) Le diagramme d'excès de deutérium du forage de PRCol suggère un changement majeur au niveau des sources d'humidité en l'an 1840, les sources océaniques chaudes y produisant le plus d'humidité. (C) Le diagramme de la force de l'ENSO montre un changement de régime à la moitié du 19^{ème} siècle. Ce changement est lié à la contribution de la dépression des Aléoutiennes plus au sud.

Figure 4 compares $\delta(^{18}\text{O})$ from the PRCol and Jellybean Lake core (Anderson, 2004; Anderson *et al.*, 2004). The JBL record comes from carbonates and reflects the meteoric water values for the average catchment elevation of about 1 600 m asl. These two 2000-year-long series are very close indeed and both show the shift at A.D. 1840 and another similar shift about A.D. 800. There are other large changes in Figure 4 but we focus on these two because they are the main shifts in level.

Three questions spring to mind. First, what is behind the sudden shift at ca. A.D. 1840 and A.D. 800? Second, why does it have different strengths in δ expression depending on ele-

vation (3.5, -0.0 and 1.5 ‰ at PRCol, Eclipse and JBL respectively). Third, and most significant, why are the PRCol and JBL records "up-side-down" with the Little Ice Age having more positive δ s when it is known from tree-ring studies that most of Alaska had a normally cold Little Ice Age.

Previous work on the 300-years NWCol core (Holdsworth *et al.*, 1992; Moore *et al.*, 2002; Rupper *et al.*, 2004) has shown that over recent times higher accumulation, which correlates in general with lower $\delta(^{18}\text{O})$, comes in years that the Aleutian Low is deeper and the vapour flow lines originate farthest south. Having "more southern moisture sources" is another way of saying that meridional vapour flow is enhanced in high accumulation years at the 5 340 m level.

Mann *et al.* (2000) point out that the statistics of ENSO (Trenberth and Hoar, 1997) undergo an important shift in the middle 19th century (Fig. 3C). They conclude that a change occurred in the mid-1800s toward fewer long warm ENSO events. From a study of tropical records, Rein *et al.* (2004) conclude that there was a major weakening in El Niño in A.D. 800. Moore *et al.* (in press) have also concluded that a major shift in tropical teleconnection occurred at ca. A.D. 1850.

MODEL SIMULATIONS FOR THE MODERN REGIME

Two isotope simulations from the coast to the Mount Logan Plateau are presented. The first uses "modern" moisture sources (including tropical), and the second only North Pacific sources.

A semi-empirical model has been used in a wide range of polar situations, in simulating $\delta(^{18}\text{O})$ and $\delta(\text{D})$ (Fisher, 1990, 1991, 1992; Kavanaugh and Cuffey, 2003). For the modern situation, the model inputs multi-latitude sources and uses measured zonal annual averages of major water cycle variables such as evaporation rate, total meridional flux, precipitable water content, precipitation, sea temperature, wind speed, relative humidity, sea ice front, etc. Figure 5A-G shows the input fields used to model the present annual global water cycle isotopes and precipitation rates at sea level (adapted from Fig. 1 of Fisher, 1990). The survival distance for water vapour (shown in Fig. 5A) is the distance that a slug of source water travels north or south before losing 63 % (1/e) of its mass by precipitation. The survival time at a given point in the water cycle is measured by the total water content divided by the precipitation rate. The survival distance is obtained by multiplying this time by the average drift velocity of the water vapour (total vapour flux/total water content). All these quantities are known and the sources of data are referenced in the captions to Figure 5. This model successfully simulates the zonal annual average sea level $\delta(^{18}\text{O})$ and d (Fig. 6A-B) as well as the precipitation rate. Figure 6C shows the simulated and measured values (Fisher, 1990, 1992).

The global model provides the initial values for a regional model that assumes moisture flux over land has a known trajectory (assumed to be perpendicular to precipitation rate isopleths) from the coast to a given site. The regional model is also empirically driven and requires precipitation, elevation and air temperature along the trajectory from the coast to the inland sites. A key element of the regional model is the weighting

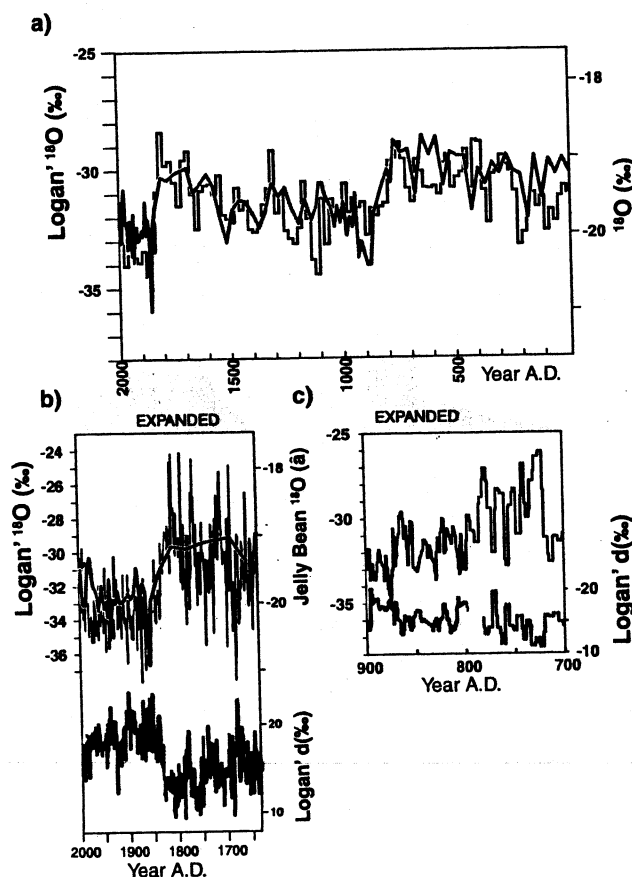


FIGURE 4. (A) The $\delta(^{18}\text{O})$ records from Jellybean Lake and from PRCol with the sudden shifts in ca. A.D. 1840 and A.D. 800 appearing in both. There is a high degree of coherence between these records. The expanded inserts (B and C) show the highest resolution data from PRCol for $\delta(^{18}\text{O})$ and annual deuterium excess (d) for the two sudden shifts. The $\delta(^{18}\text{O})$ shift near A.D. 800 in C appears to have happened in less than 5 years and, like that in A.D. 1840, it is accompanied by a change in the level of the deuterium excess.

(A) Les séries temporelles de $\delta(^{18}\text{O})$ du lac Jellybean et du forage de PRCol montrent des changements brusques en ca. A.D. 1840 et A.D. 800, une forte cohérence existant entre les séries temporelles. Les changements observés dans $\delta(^{18}\text{O})$ et l'excès de deutérium annuel (d) du forage de PRCol sont illustrés à une haute résolution (en B et C). Le changement qui s'est produit en A.D. 800 dans la série $\delta(^{18}\text{O})$ s'est effectué en moins de cinq années et, comme en A.D. 1840, il est accompagné d'un changement dans l'excès de deutérium.

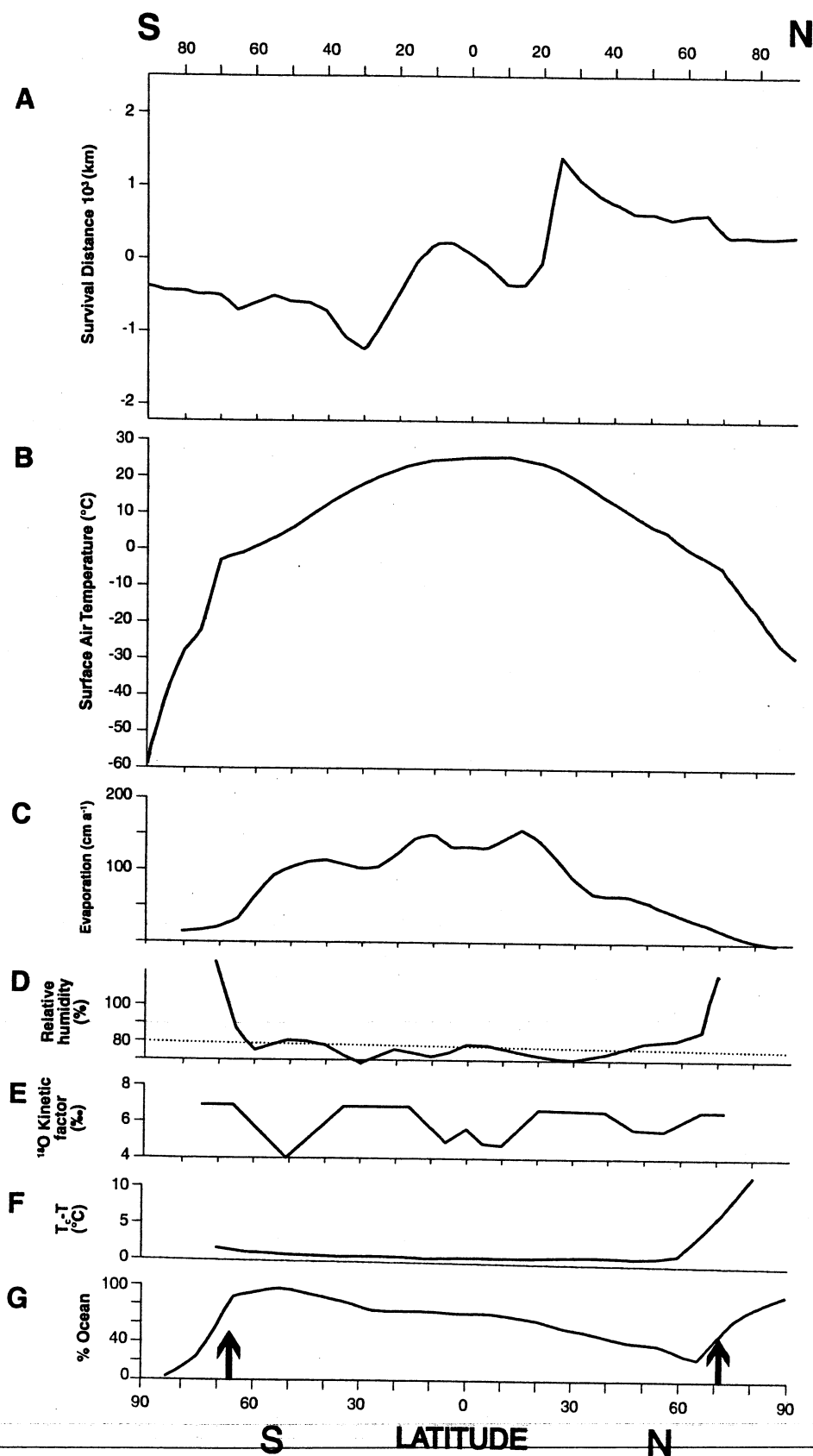


FIGURE 5. Modern annual zonal averages of annual input data fields for the isotope model, and model output compared to observations (Fisher, 1990). (A) Survival distance (km) for water vapour (I). Positive values indicate northward vapour transport. (B) Near-surface air temperature (T). (C) Evaporation rate (E). (D) Near-surface relative humidity (h). (E) Kinetic evaporation fractionation factor (k_{18}). (F) Difference between the sea temperature and the air temperature ($T_e - T$). (G) Percentage of the zone that is ocean with vertical arrows indicating the annual average position of the sea ice.

Moyennes annuelles des intrants et des extrants du modèle isotopique comparées aux observations (Fisher, 1990). (A) Distance de survie (km) de la vapeur d'eau (I). Les valeurs positives indiquent un transport vers le nord. (B) Température de l'air près de la surface (T). (C) Taux d'évaporation (E). (D) Humidité relative près de la surface (h). (E) Facteur de fractionnement d'évaporation cinétique (k_{18}). (F) Différence entre la température de la mer et la température de l'air ($T_e - T$). (G) Pourcentage de la zone dans l'océan (C), les flèches verticales indiquant la position relative des glaces de la mer.

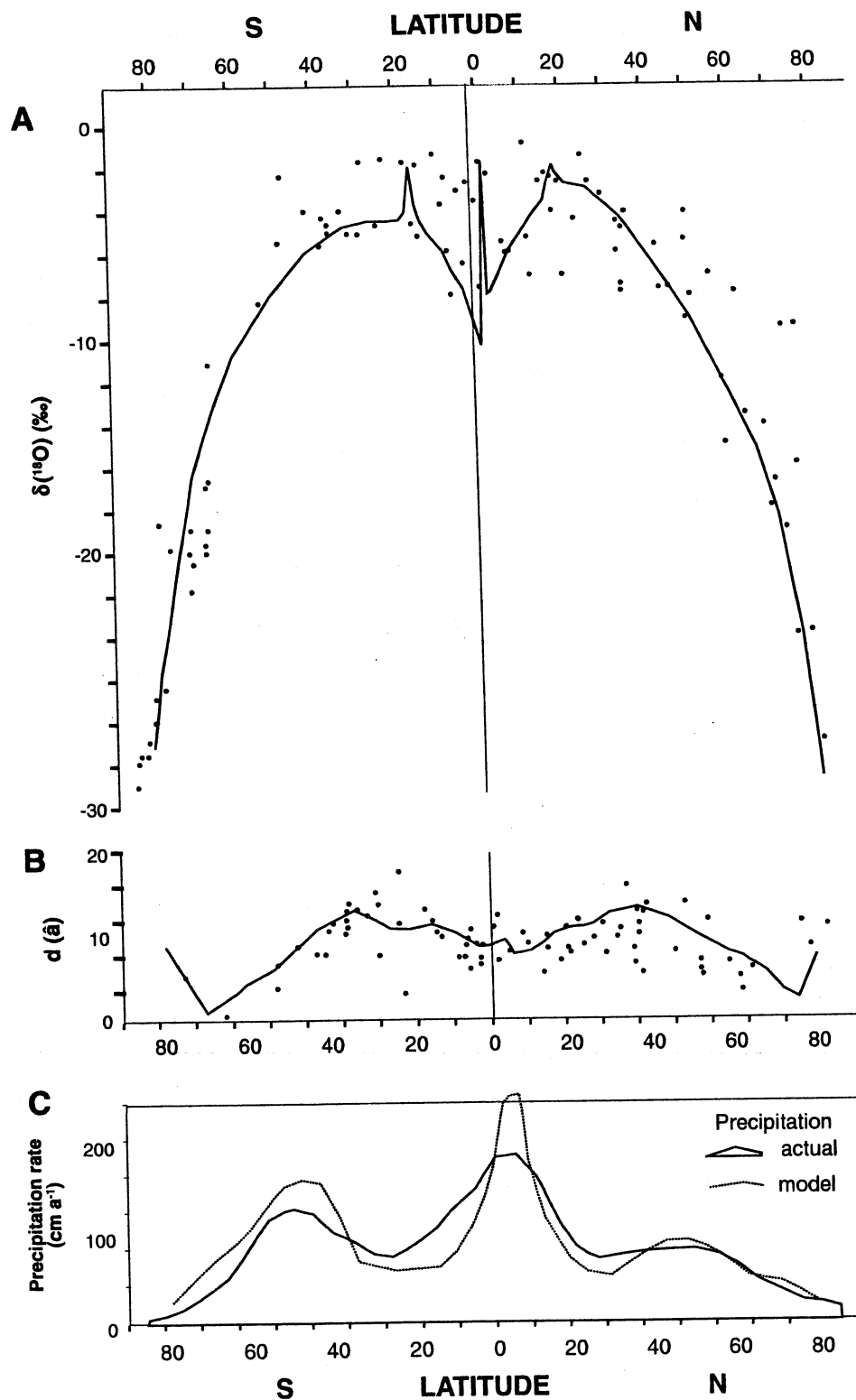


FIGURE 6. (A) Annual averages of $\delta(^{18}\text{O})$ versus latitude. The dots are measured values and the line comes from the global model. (B) Annual deuterium excess, d , with dots from the measured data and the line from the model. The global runs assume ice clouds form at -10°C , and the supersaturation in these clouds is after Jouzel *et al.* (1984) and Fisher (1990). (C) The measured annual zonal precipitation rates (shaded) and the model's calculations.

(A) Moyennes annuelles de $\delta(^{18}\text{O})$ en fonction de la latitude. Les points sont les valeurs mesurées et la ligne est issue du modèle global. (B) Excès de deutérium annuel, d , où les points sont les valeurs mesurées et la ligne est issue du modèle global. La simulation présume que les nuages se forment à -10°C et que leur supersaturation se fait d'après Jouzel *et al.* (1984) et Fisher (1990). (C) Taux de précipitation annuels mesurés (ombragé) et calcul du modèle.

given to moisture sources depending on the elevation of the site. Higher sites receive less local water. The weighting factor used in the model preserves the vertical profile for total precipitable water and simulates the need for a certain horizontal fetch L_x ($= 2\,500$ km), to get moisture from a given ocean

source up to elevation L_z ($= 3\,000$ m asl) (Fisher, 1990). Examples of the regional model output for traverses up the Devon Island Ice Cap and up a line in East Antarctica are given in Figure 7 (Fisher, 1990). In both cases the accumulation rates and elevations are well known.

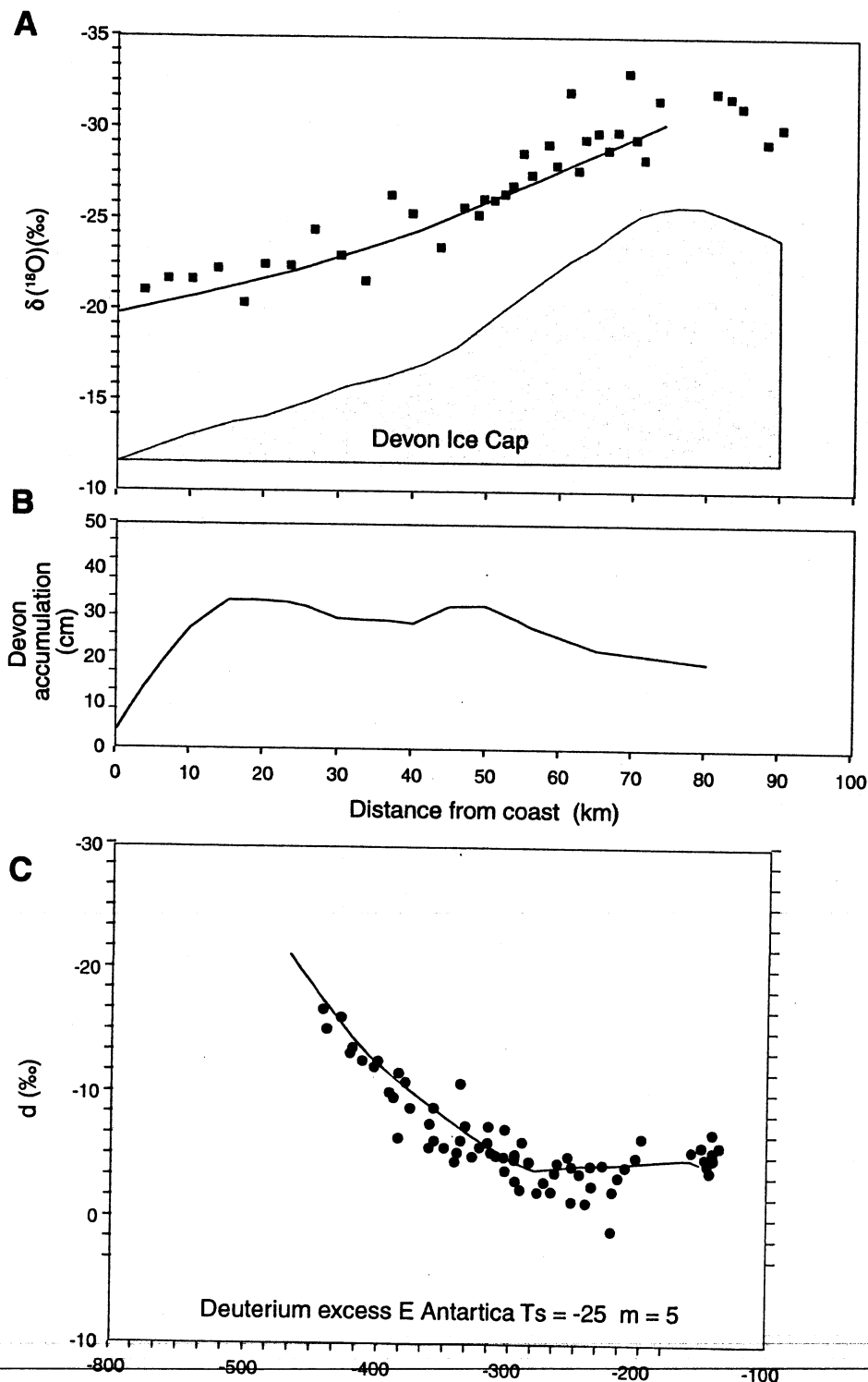


FIGURE 7. Example of the regional model for the southeast side of the Devon Ice Cap. (A) The $\delta(^{18}\text{O})$ data (squares) and (B) the accumulation rate data (both from Koerner and Russell, 1979). The lines are predicted by the model (Fisher, 1992) using the measured accumulation and coastal values of $\delta(^{18}\text{O})$. (C) An East Antarctic example of d versus $\delta(^{18}\text{O})$ from observations (Petit *et al.*, 1991). The line is that predicted by the regional model (Fisher, 1992).

*Exemple du modèle régional pour la partie sud-est de la calotte glaciaire de Devon. (A) Données de $\delta(^{18}\text{O})$ (carrés) et (B) taux d'accumulation (d'après Koerner and Russell, 1979). Les lignes sont prédites par le modèle (Fisher, 1992) à partir des accumulations mesurées et les valeurs côtières de $\delta(^{18}\text{O})$. (C) Exemple de d en fonction de $\delta(^{18}\text{O})$ à partir des observations (Petit *et al.*, 1991) pour l'est de l'Antarctique. La ligne représente la prédiction du modèle régional (Fisher, 1992).*

The accumulation rate data, average temperatures and moisture trajectories are not as well known in the St. Elias Mountains. However, substantial data from the "Icefield Ranges Research Project" along with new data gathered by us, may be used as an input set for the regional runs. Figure 8 (adapted from Marcus and Ragle, 1972) shows the accumulation rates and elevations from Yakutat to Kluane Lake. The expanded part (Fig. 8B) gives the data from Seward Glacier to the top of the Logan Plateau. The runs start at the coast (Yakutat) and end on the Logan Plateau. The annual average temperature at Yakutat, and our camps at Quinto Sella (QS), Eclipse (E), KingCol and PRCol are known. Linear interpolation with elevation is used in between. The Marcus and Ragle (1972) data have a blank between Yakutat and kilometre 60 in Figure 8A. The three numbered lines are each used as part of three input sets to the regional model, and the spread of results imposes uncertainty on the simulations. Another source of uncertainty in the input data is the value of the coastal precipitation rate, as the global model can only produce zonal averages of $\delta(^{18}\text{O})$ and precipitation at the coast.

The modern predicted $\delta(^{18}\text{O})$ and d from coastal Alaska to the top of Logan appear in Figures 9A and 9B as line-functions. The shaded areas show the range of simulations due to the geographic paucity and temporal spread in measured local accumulation rates. The points in Figure 9A represent measured $\delta(^{18}\text{O})$ of surface samples taken over a wide range of years. Most of these points are from Holdsworth and Krouse (2002). This model captures most of the vertical $\delta(^{18}\text{O})$ structure, including the recumbent section from about 3.0 to 4.8 km. Similarly Figure 9B presents the modeled and measured d for modern annual conditions. For each site (or elevation), the model output includes the relative importance of source ocean strips (*i.e.* the percentage of the accumulation that originates in each source ocean strip). For the modern regime (defined by Figs. 5 and 8), the source ocean contribution distributions for sites at 5 400, 2 900 and 1 300 m asl are plotted in Figure 10A. The PRCol site (5 400 m asl) normally receives relatively little local water, whereas the 1 300 m asl sites receive quite a lot.

MODEL EXPERIMENT FOR PURELY ZONAL SOURCES OF WATER

As an experiment to test the importance of water source, the model was fed only with water originating in the North Pacific strip defined by latitude range 40 °N to 60 °N and from the coast of Asia to North America (longitude 120 °E to 120 °W), with all southerly sources excluded. The empirical water cycle variables averaged in 15 longitude bins within this North Pacific rectangle are the input for the "pure zonal source" model run. Figure 11 shows the input data for this North Pacific source strip. In this experiment the survival distance (Fig. 11G) is found using only the zonal water vapour flux (Fig. 11E). This approach is taken to get the input "empirical data" *in lieu* of having the actual variables for a hypothetical pure zonal flow. As before, the model predicts the precipitation rate as a by-product. The dashed line in Figure 11C is the model precipitation rate and the step function is the measured rate. The size and first-order fit are good. The differences are due to the

way the zonal regime has been constructed. Also the modeled precipitation requires mass conservation within its set of strips, clearly not met in this experiment. The overland portion of the model run assumes the same input accumulation and temperature fields as were used before in the St. Elias.

The main difference between the modern-ocean-source runs and this experimental North Pacific zonal-source is that the former includes tropical water with surface temperatures up to +30 °C and a weighted source mean of about +19 °C, whereas the latter experimental zonal run has ocean water strips all with temperatures about +10 °C. We hypothesize that, prior to A.D. 1840, North Pacific water fed the St. Elias via zonal flow, and that thereafter, modern flow has delivered water from all the possible southern latitudes. Because of the uncertainties in the hypothetical North Pacific input, a run is picked within these uncertainties to hit the pre-A.D. 1840 $\delta(^{18}\text{O})$ average at PRCol. After this "fine tuning", the simulation for all the other elevations is objective. Figure 12A shows the simulated relation of $[\delta(\text{zonal}) - \delta(\text{modern})]$ versus elevation. The line is from the model runs and the squares are data from the three sites. The shading reflects the estimate of errors taken from Figure 9A. The PRCol difference is successfully modeled (as it must be), but what is pleasing is the success that the [model + hypothesis] has in forecasting the differences at the Eclipse and Jellybean Lake sites. Eclipse has virtually no shift and JBL has a 1.5‰ shift. The twin maxima at 1.3 and 5.4 km elevation in Figure 12A come about because of source weighting factors (Fig. 10). Switching the water source from warmer tropical to cooler northern oceans should affect the d substantially (Johnsen *et al.*, 1989), as is confirmed by the modeled difference of $[d(\text{zonal}) - d(\text{modern})]$ versus elevation (Fig. 12B). The difference is rather insensitive to elevation. The only value available for the d -difference is from PRCol, but the predicted shift is very close to the 4.5‰ shown in Figure 3C. This is a further validation of the hypothesis.

The level shift at A.D. 800 shown in the PRCol and JBL plots of Figure 4 is probably another regime shift. The PRCol Holocene record contains many such sudden and large shifts in $\delta(^{18}\text{O})$ and d on the order of ~3 to 5 ‰.

This moisture source switch hypothesis can successfully reproduce abrupt changes in $\delta(^{18}\text{O})$ at 5 400 and 1 300 m asl with no change at 3 000 m asl and changes in d at all elevations. However, other processes could also cause sudden changes. Scouring away of winter snow before A.D. 1840 could have produced similar changes (Fisher *et al.*, 1983), as could wind-enhanced vapour transport and isotopic fractionation (Neumann and Waddington, 2004). Both could be accomplished at the PRCol site by a sudden and systematic decrease in wind speeds at ca. A.D. 1840. This explanation would also apply to the older Logan core (Fig. 1C) that has a similar $\delta(^{18}\text{O})$ shift, but it would fail to explain the change at Jellybean Lake or the zero shift at Eclipse. Given the amplitude of the seasonal $\delta(^{18}\text{O})$ variation on the Logan Plateau, about 40 ‰ of the year's accumulation (all the winter snow) would have to be removed to achieve a 3‰ shift in the annual average (Fisher *et al.*, 1993). There is no such large sudden change in the accumulation rate on the Plateau in the mid-1800s (Holdsworth *et al.*, 1992). In any case, hypothesizing such a sudden change in

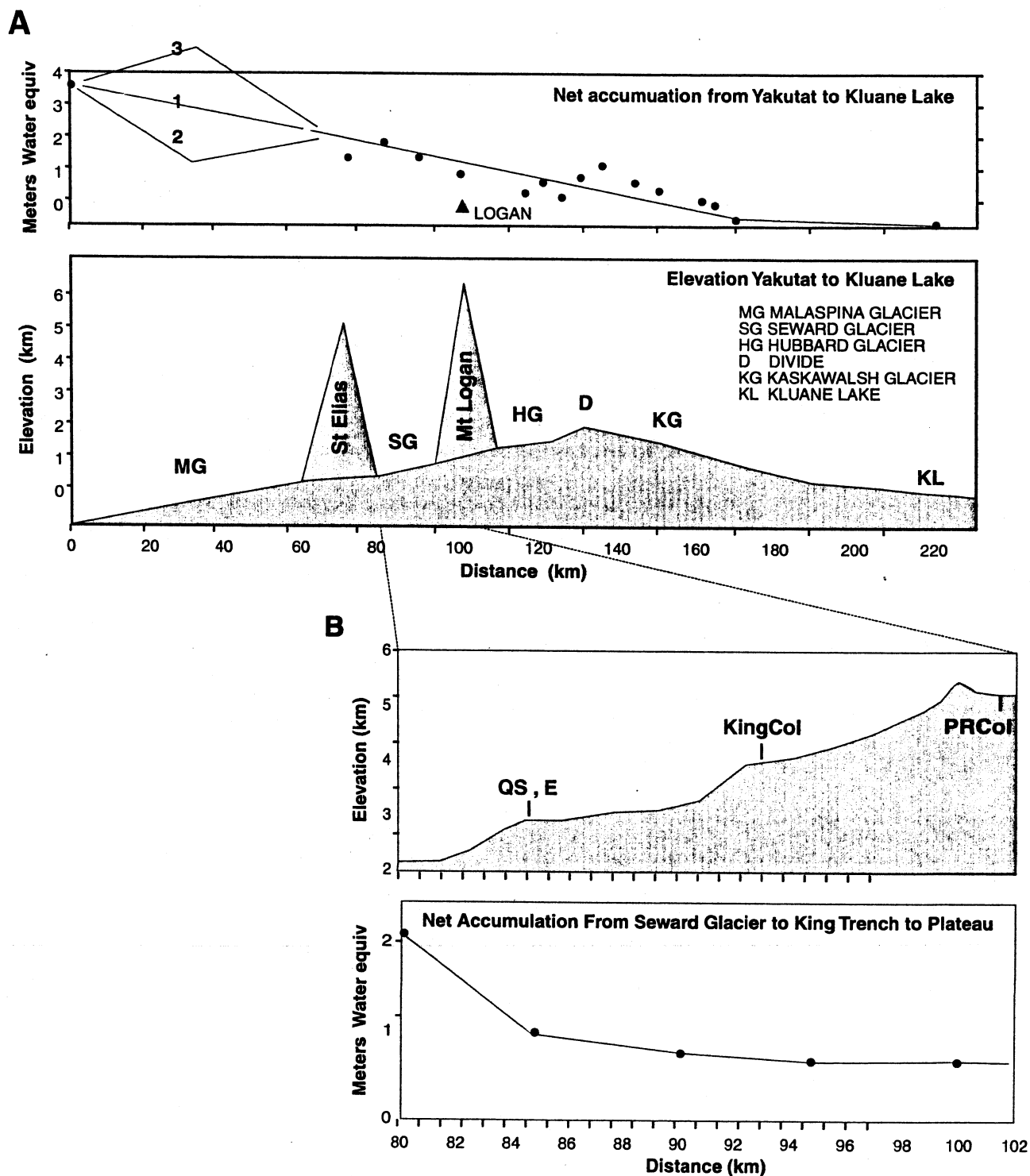


FIGURE 8. Regional input fields for "modern" St. Elias area. (A) Accumulation rates and elevations along a traverse from Yakutat on the Alaskan coast to Kluane Lake in the Yukon. (B) The expanded section from the upper Seward Glacier to the Mount Logan Plateau. The figure is adapted from Marcus and Ragle (1972) and supplemented with data collected by us in the Mt. Logan area. Ideally, model input of these variables should be along a traverse perpendicular to the accumulation isopleths. This ideal is not attainable, but the model results are close.

Intrants pour la région du mont St-Elias. (A) Taux d'accumulation et altitudes d'un transect entre Yakutat, sur la côte de l'Alaska, et le lac Kluane, au Yukon. (B) Agrandissement du transect entre le Haut Glacier de Seward et le Plateau du mont Logan. Cette figure est une adaptation de Marcus and Ragle (1972), combinée avec les données collectées par les auteurs dans la région du mont Logan. Idéalement, les intrants du modèle devraient être perpendiculaires aux lignes d'accumulation. Cet idéal est difficile à atteindre, mais les données utilisées dans le modèle s'en rapprochent.

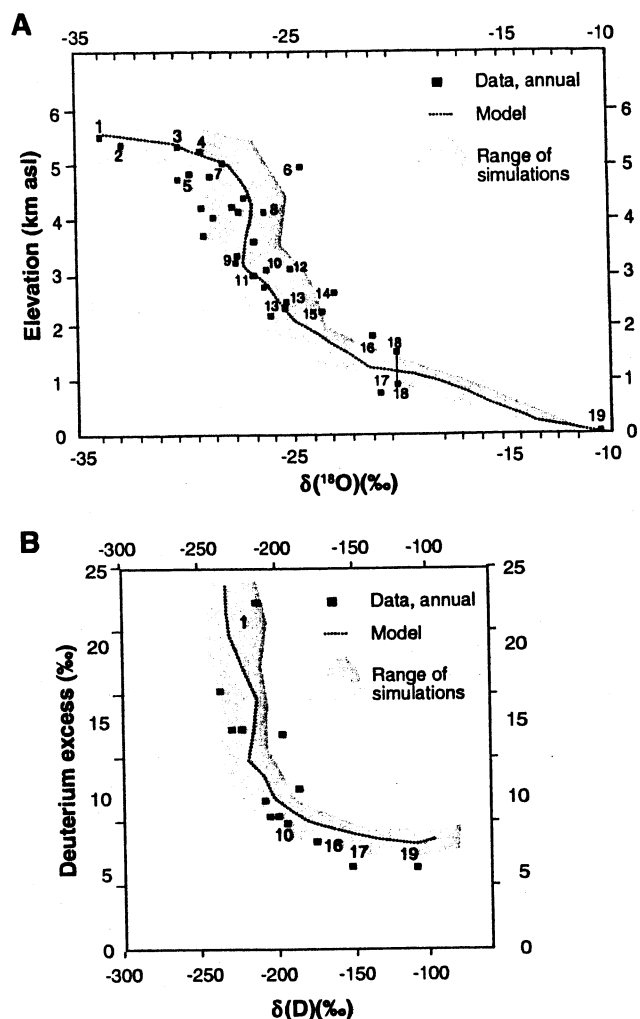


FIGURE 9. (A) Measured and modeled $\delta(^{18}\text{O})$ for the St. Elias Mountains and South Yukon-Alaska. The shaded area shows the range of possible model results allowed by the uncertainties in the input data to the empirical model, and the line is the "best run". (B) Measured and modeled deuterium excess. The model used "modern" moisture delivery to the sites. The data points are from many researchers: (1) Logan Plateau (GH), (2) Prospector-Russell Col (GSC), (3) Logan Plateau PRNN (GH), (4) Windy Camp (GH), (5) Mount Churchill (GH), (6) Mount Bona (GH), (7) Football Field (GH), (8) King Col (GH, KA), (9) King Trench (GH, KA), (10) Eclipse (Kreutz, Yalcin, Wake), (11) Quintino Sella (GH, GSC), (12) (GH), (13) Rusty Glacier, (14) Divide (GH), (15) (GH), (16) Seward Glacier (GH), (17) Whitehorse, (18) Jellybean Lake, (19) Yakutat (GH). (GH = Gerrold Holdsworth; KA = Kumiko Azuma; GSC = Geological Survey of Canada).

(A) $\delta(^{18}\text{O})$ mesuré et modélisé pour les montagnes St-Elias et le sud du Yukon-Alaska. La région ombragée montre les résultats possibles du modèle empirique liés aux incertitudes des intrants, et la ligne représente la meilleure simulation. (B) Excès de deutérium mesuré et modélisé, le modèle utilisant la production moderne d'humidité disponible aux sites. Les données proviennent de différents chercheurs: (1) Logan Plateau (GH), (2) Prospector-Russell Col (GSC), (3) Logan Plateau PRNN (GH), (4) Windy Camp (GH), (5) Mount Churchill (GH), (6) Mount Bona (GH), (7) Football Field (GH), (8) King Col (GH, KA), (9) King Trench (GH, KA), (10) Eclipse (Kreutz, Yalcin, Wake), (11) Quintino Sella (GH, GSC), (12) (GH), (13) Rusty Glacier, (14) Divide (GH), (15) (GH), (16) Seward Glacier (GH), (17) Whitehorse, (18) Jellybean Lake, (19) Yakutat (GH) (GH = Gerrold Holdsworth; KA = Kumiko Azuma; GSC = Geological Survey of Canada).

the wind is a large assumption, and it fails to explain the changes at the lower sites. Sudden and persistent changes in wind speeds might also be expected to be reflected in the chemical and dust loading in the ice cores. There is no such persistent change in the Eclipse core at A.D. 1840.

The moisture-switch hypothesis explains the relative changes at the three elevations, while the other processes do not. We will continue forward with this hypothesis. Holdsworth (2001) has examined the "statistical black box" of our simple model and attempted to model the vertical structure of the low pressure systems that deliver moisture storm by storm. His synoptic modeling will no doubt continue to throw more light on the detailed process changes that occur during one of the hypothesized regime changes.

DISCUSSION

The stable isotopes from the ice and lake cores from the southern Yukon paint a consistent picture of the Pacific water cycle over the last 2000 years and over a vertical range of nearly 5 000 metres. In this part of North America $\delta(^{18}\text{O})$ is not a measure of past temperature but a "source-meter," indicating the relative connectedness of these Northern sites to either tropical or North Pacific water sources. There are sudden steps in $\delta(^{18}\text{O})$ at PRCol and Jellybean Lake (e.g. A.D. 1840 and A.D. 800) which can be modeled by switching between modern-like and zonal water sources for these sites. What happens on Mount Logan and at JBL has some connection with the tropical winds. In the middle 1800s there is a shift in the statistics of ENSO (Fig. 3C; Mann *et al.*, 2000) from numerous long persistent warm-event ENSOs to fewer after A.D. 1850. Prior to A.D. 1840 we hypothesize mainly zonal source water for our suite of sites. This suggests that the North Pacific was more isolated from the south during the Little Ice Age, with a more southerly Polar Front (Mayewski *et al.*, 1994). The shift from zonal to modern vapor flux appears to have been abrupt and may have signaled the end of the Little Ice Age.

Thompson *et al.* (1986) and Hendy *et al.* (2002) have shown that pre-A.D. 1850, the trade winds in the Pacific were stronger than post-A.D. 1850 and (Moore *et al.*, in press) have related these stronger trade winds to a stronger Walker and weaker Hadley circulation pre-A.D. 1850. Similar conclusions are drawn from Antarctic ice cores (Mayewski *et al.*, 2004). The Eclipse accumulation rate is in anti-phase with that at the 5 400 m asl site, further demonstrating the vertical structure in water vapour transport (Yalcin *et al.*, 2004).

From a study of sea-ice-rafted debris from a core from the Emerald Basin off Nova Scotia, Keigwin *et al.* (2003) inferred that at about A.D. 1850 the export route of sea ice from the Arctic Ocean shifted from the west to the east side of Greenland. They hypothesize that a shift in the position of the "Icelandic Low" was responsible for this change. Meeker and Mayewski (2002) also attribute late Little Ice Age northern paleo-climate changes to shifts in the main pressure centres.

Another abrupt shift occurred in A.D. 800. Rein *et al.* (2004) find a very abrupt change in tropical and mid-latitude paleo-moisture archives of both hemispheres in A.D. 800 and suggest that there were persistent weak El Niños between A.D. 800 and A.D. 1250 that were related to the Medieval

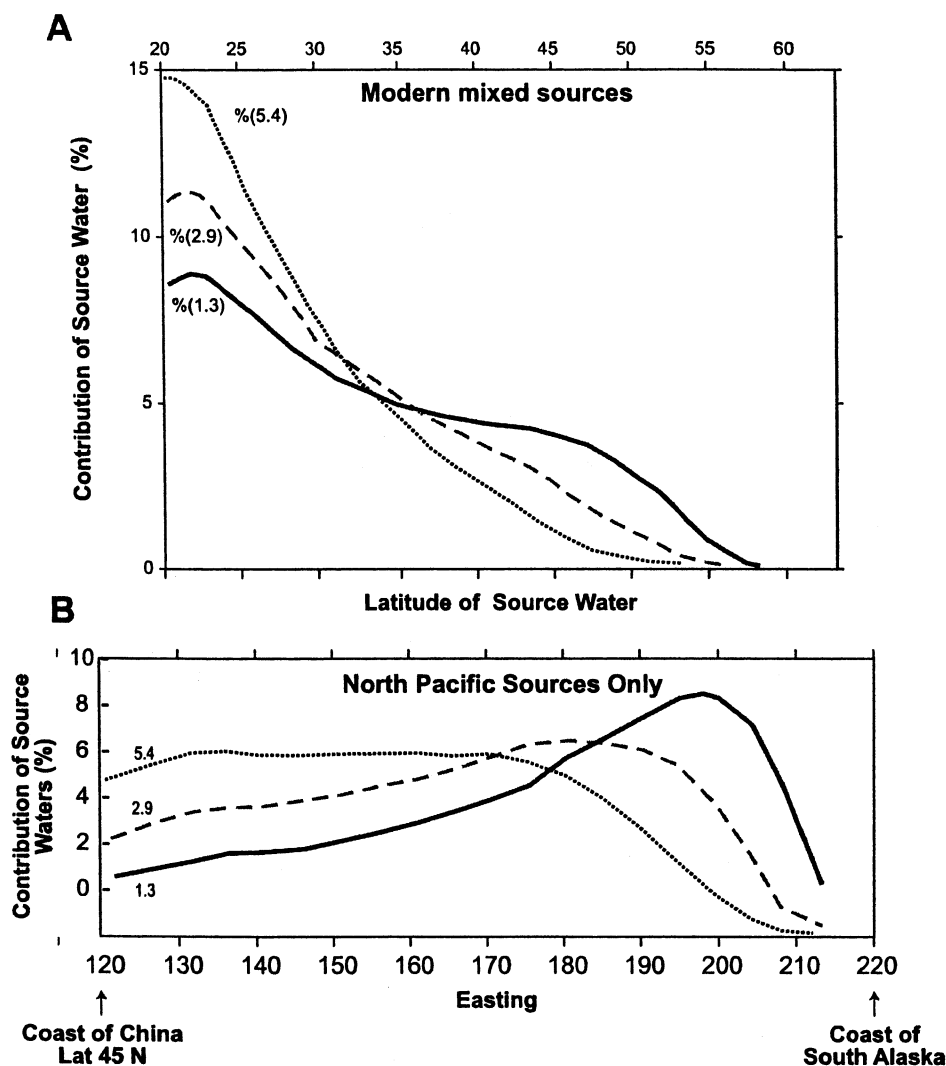


FIGURE 10. The moisture source contributions at three elevations in the St. Elias, assuming modern and "pure" North Pacific zonal regimes. It is these weighting functions that largely determine the modeled differences in stable isotope signatures at the various elevations. (A) Moisture source weightings for the modern regime and (B) for the purely North Pacific source strip experimental regime.

Les sources d'humidité à trois altitudes des monts St-Elias, le régime de la zone du Pacifique nord y étant considéré comme moderne et "pur". Ce sont les fonctions pondérées qui déterminent les différences observées dans les signatures isotopiques modélisées aux trois altitudes choisies. (A) Sources d'humidité pondérées pour le régime moderne et (B) le régime expérimental du Pacifique nord.

Warm Period. Their best-resolved series, a Peruvian drought index (from core 106KL), suggests the regime shift at A.D. 800 took less than a few decades. At PRCol, this shift occurred in less than 10 years (Fig. 4C). The period between A.D. 800 and A.D. 1250 was dry in Peru whilst core evidence from the Cariaco Basin core (ODP1002) suggests the Venezuelan side of South America was moist (Rein *et al.*, 2004).

The contention that the Northern Hemisphere wind-moisture flux system underwent a change in the middle of the 19th century seems to be consistent with many independent studies. The present work suggests that the changes in the mid-19th century leave their mark in the St. Elias-South Yukon $\delta(^{18}\text{O})$ records because they are moisture source (not temperature) histories. The PRCol $\delta(^{18}\text{O})$ history, having the best resolution, shows that these regime shifts happened quickly, within a year in the case of the A.D. 1840 shift. Preliminary analysis of the PRCol $\delta(^{18}\text{O})$ Holocene record suggests that it contains many such shifts.

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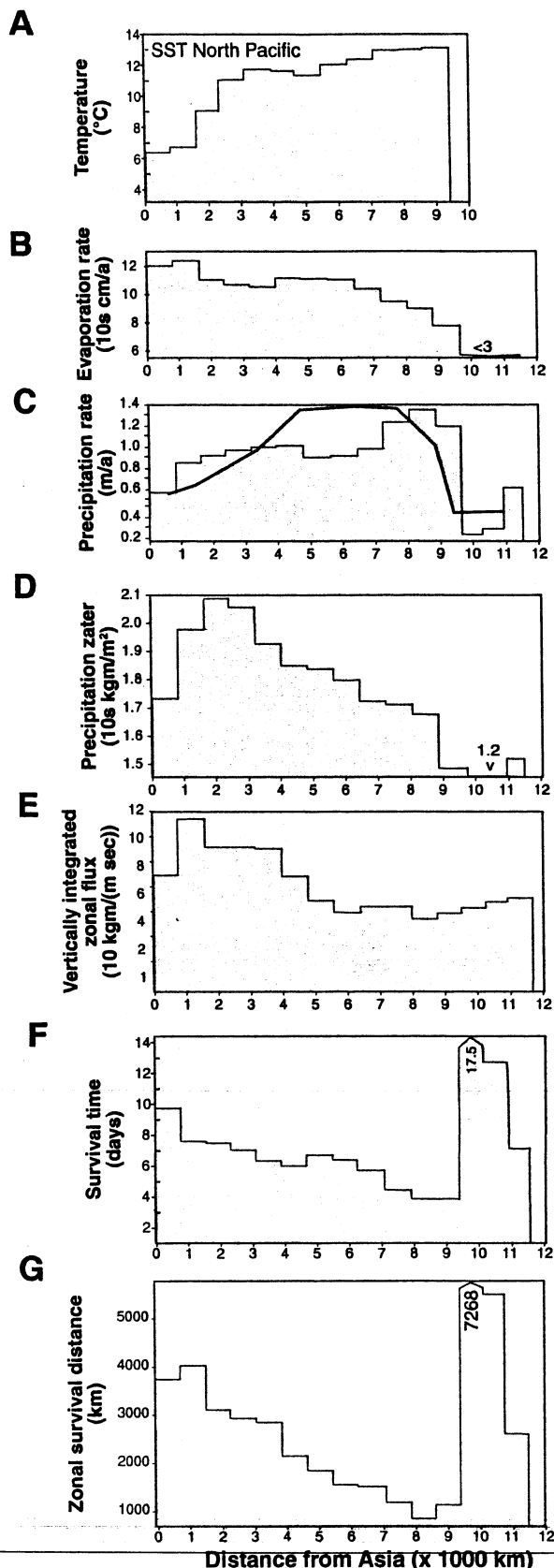


FIGURE 11. Empirical input data fields for the North Pacific Ocean strip from 40°N to 60°N and longitude range 120°E to 120°W. (A) Modern annual sea surface temperatures. (B) Evaporation rate annual averages (Peixóto and Oort, 1983). (C) Precipitation rate (Jaeger, 1983). The line is a model output, matching the input reasonably well given that mass conservation is not maintained in the numerical experiment that was carried out. (D) Total precipitable water in the atmosphere. (E) Measured annual average zonal water vapour flux (Peixóto and Oort, 1983). (F) Annual survival time (total precipitable water/precipitation rate), using Figures 10C and D. (G) Annual survival distance (zonal water vapour flux/precipitation rate) using Figures 10C and E. See Fisher (1990, 1992) for more details and explanations of the model input variables.

Données empiriques de l'Océan Pacifique nord entre 40°N et 60°N et entre 120°E et 120°W. (A) Températures annuelles modernes de la surface de la mer. (B) Taux d'évaporation annuels moyens (Peixóto and Oort, 1983). (C) Taux de précipitation (Jaeger, 1983). La ligne représente l'extrait du modèle et correspond très bien avec les intrants. (D) Total de l'eau précipitable dans l'atmosphère. (E) Flux de vapeur d'eau annuel moyen mesuré (Peixóto and Oort, 1983). (F) Temps de survie annuel (total de l'eau précipitable/taux de précipitation), estimé à partir des figures 10C et D. (G) Distance de survie annuelle (flux de vapeur d'eau/taux de précipitation), estimé à partir des figures 10C et E. Voir Fisher (1990, 1992) pour obtenir les détails et les explications sur les intrants du modèle.

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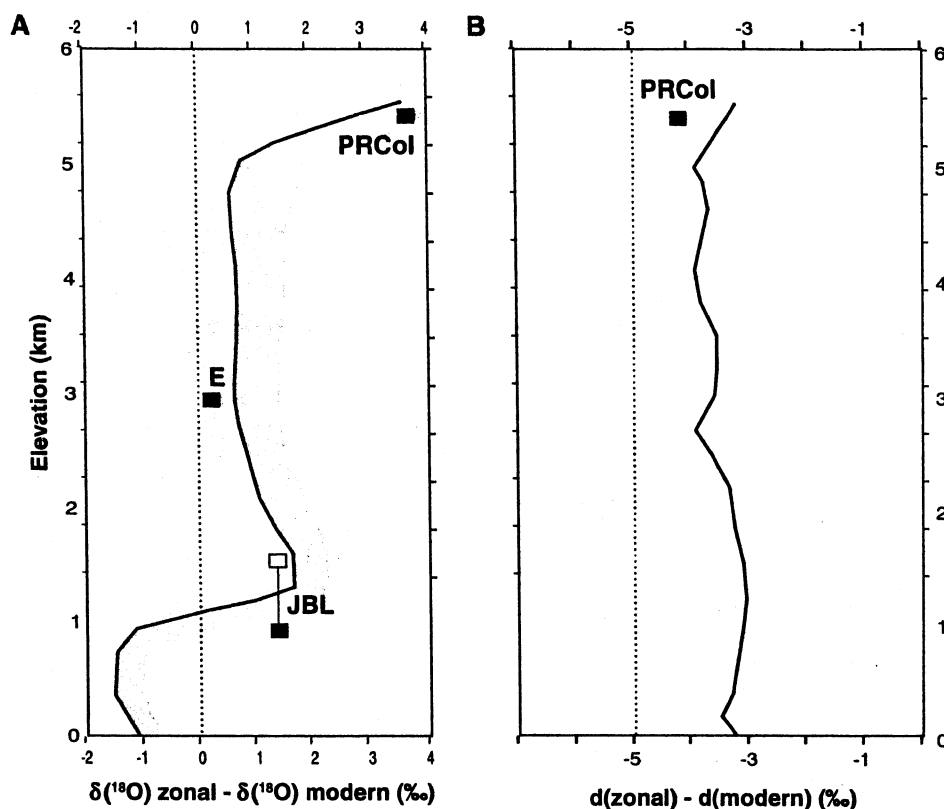


FIGURE 12. Predicted effects on (A) $\delta(^{18}\text{O})$ and (B) d of shifting from a zonal (only North Pacific) moisture source to modern moisture sources (including tropical). The squares are the differences between pre- and post-A.D. 1840 $\delta(^{18}\text{O})$ and d values in Figures 3 and 4 for the PRCol, Eclipse and Jellybean Lake sites. The shading around the $[\delta(\text{zonal}) - \delta(\text{modern})]$ versus elevation profile indicates the estimated errors taken over from Figure 5 and are mostly due to the lack of model-input accumulation data now and in the past. The elevation of Jellybean Lake is 800 m asl, but because it is in a valley surrounded by nearby mountains that provide runoff, the effective elevation is 1 650 m asl.

Effets prédits sur (A) $\delta(^{18}\text{O})$ et (B) d du changement d'une source d'humidité zonale (Pacifique nord seulement) à des sources d'humidité modernes (incluant les tropicales). Les carrés représentent les différences entre $\delta(^{18}\text{O})$ pré- et post-A.D. 1840 et les valeurs de d dans les figures 3 et 4 pour les sites PRCol, Eclipse et lac Jellybean. L'ombragé autour de $[\delta(\text{zonal}) - \delta(\text{moderne})]$ en fonction du profil d'élévation indique que les erreurs d'estimation issues de la figure 5 sont principalement dues au manque de données actuelles et passées dans le modèle. L'élévation du lac Jellybean est de 800 m asl, mais puisqu'il est situé dans une vallée entourée de montagnes qui contribuent au ruissellement, l'altitude effective est de 1 650 m asl.

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To: rscnmunk@giss.nasa.gov

Cc: gschmidt@giss.nasa.gov

Subject: [Fwd: The physics of Climate modelling / Version française]

Date: 01/11/07 12:13:11

Hi Rob,

The 'Physics of Climate Modelling' piece has been translated into French. I talked to AIP and they are ok with this appearing on the GISS website (as long as it comes with the Copyright notice and link to the original). Can I ask you to make this up as a html page and link it to the English original? (If you prefer that I do it, just let me know). Thanks

Gavin

----- Original Message -----
Subject: The physics of Climate modelling / Version française
From: Olivier Daniélo <olivierdanielo@gmail.com>
Date: Thu, January 11, 2007 11:12 am
To: contrib@realclimate.org
garidel@cerege.fr

Voici la traduction d'Yves Fouquart (je vous transmets le bonjour de sa part).

Bien à vous,
Olivier

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http://www.physicstoday.org/vol-60/iss-1/72_1.html*<<http://www.physicstoday.org/vol-60/iss-1>,

La physique de la modélisation du climat
Traduit par Yves Fouquart

Le climat est un phénomène de grande échelle qui résulte des interactions complexes entre des systèmes physiques de petite échelle. Cependant, en dépit de cette complexité, les modèles climatiques ont fait preuve de quelques succès impressionnants.

Les projections climatiques effectuées avec des modèles numériques sophistiqués ont permis d'informer les politiques du monde entier des dangers potentiels des interférences de l'activité humaine avec le système climatique. Ces codes numériques prétendent modéliser une grande partie du système. Mais quelle est la physique de ces modèles, comment sont ils évalués et à quel point sont ils crédibles ?

La tâche que les modélisateurs du climat se sont assignée est de réunir toutes leurs connaissances des interactions locales des masses d'air et d'eau, de leur vitesse et de l'énergie et d'utiliser cette connaissance pour expliquer les caractéristiques de grande échelle du système climatique, sa

d'une tâche formidable et loin d'être terminée, les succès obtenus jusqu'ici sont pourtant surprenants. En conséquence, les climatologues ont quelques bonnes raisons de croire que ce n'est pas une tentative complètement folle.

La modélisation du climat dérive des efforts qui furent d'abord formulés dans les années 1920 pour la prédiction numérique du temps météorologique. Ce n'est, cependant, qu'à partir des années 1950 que les ordinateurs ont acquis une puissance suffisante pour permettre une description minimale des systèmes météorologiques. Depuis lors, les modèles se sont complexifiés sans cesse en incorporant de plus en plus de composants du système : surfaces continentales, océans, glace et, plus récemment, des représentations interactives des aérosols, de la chimie atmosphérique et du cycle du carbone. En fait, le travail de recherche interdisciplinaire mené pour comprendre le changement climatique est, en grande partie, piloté par le développement des modèles. Les modèles actuels sont des outils flexibles, capables de répondre à une large variété de questions, mais cela a un prix : ils en deviennent presque aussi difficile à analyser et à comprendre que le monde réel.

La physique de base, les propriétés émergentes

La physique des modèles du climat peut être divisée en trois catégories. La première inclut les principes fondamentaux tels que la conservation de l'énergie, de la vitesse et de la masse et les processus tels que ceux de la mécanique orbitale qui peuvent être calculés à partir des principes fondamentaux. La deuxième inclut une physique bien connue en théorie, mais qui, en pratique, doit être approximée à cause de la discrétisation d'équations continues. On peut citer comme exemples le transfert de rayonnement à travers l'atmosphère et les équations de Navier Stokes qui décrivent le mouvement des fluides. La troisième catégorie concerne une physique connue empiriquement telles les formules qui décrivent l'évaporation comme une fonction de la vitesse du vent et de l'humidité.

Pour les deux dernières catégories, les modélisateurs développent souvent des paramétrisations qui essaient de capter les phénomènes essentiels du processus de petite échelle. Par exemple, la moyenne de la nébulosité pour une boîte de 100 km² n'est pas directement reliée à la moyenne de l'humidité dans la boîte. Cependant, dans la mesure où l'humidité moyenne augmente, la nébulosité moyenne augmente aussi. Cette relation monotone pourrait être la base d'une paramétrisation, bien que les schémas actuels soient significativement plus complexes que cet exemple.

Étant donnée la nature des paramétrisations entre autres caractéristiques, un modèle climatique requiert diverses expertises. De ce fait chaque modèle aura ses propres détails qui lui sont uniques. Cependant, une grande partie des propriétés prévues par les modèles climatiques est robustes en ce sens qu'elle ne dépend pas de façon significative des paramétrisations spécifiques et de leur représentation spatiale.

La propriété la plus intéressante du système climatique est l'émergence. C'est-à-dire que les phénomènes de grande échelle ne sont pas simplement des fonctions triviales de la physique de petite échelle mais qu'ils résultent de la complexité du système. Par exemple, aucune formule ne décrit la zone de convergences intertropicales qui résulte d'une combinaison du cycle saisonnier du rayonnement solaire, des propriétés de la convection humide, de la rotation de la Terre, et ainsi de suite. Ces qualités émergentes font que la modélisation climatique est fondamentalement différente de la résolution numérique d'équations délicates.

La modélisation du climat est aussi fondamentalement différente de la prévision du temps. Le temps météorologique est un problème de valeur initiale : étant donné la situation d'aujourd'hui quelle sera la situation de demain ? Le temps météorologique est chaotique ; des différences imperceptibles dans l'état initial de l'atmosphère conduisent à des conditions radicalement différentes une semaine ou deux plus tard. Par contre, le climat est un problème de conditions aux limites - une description statistique de l'état moyen et de la variabilité d'un système, et non pas un chemin individuel à travers l'espace des phases. Les modèles climatiques actuels conduisent à des climats stables et non chaotiques ce qui implique que les questions qui regardent la sensibilité du climat à, disons une augmentation des gaz à effet de serre sont bien posées et justifient de l'utilisation des modèles. Cependant, dans la mesure où sont incorporés davantage de composants compliqués tels que les systèmes biologiques, la dynamique complète des calottes glaciaires, etc., il est concevable que les possibilités de contre réaction s'accroissent et que des climats chaotiques en résultent.

Tests des modèles climatiques

L'évaluation des modèles s'effectue sur deux niveaux distincts - la petite échelle à laquelle on évalue les spécificités d'une paramétrisation et la grande échelle sur laquelle on peut tester les propriétés émergentes telles qu'elles sont prédites. Le socle primaire des tests est le climat de l'époque présente particulièrement depuis 1979 quand une quantité significative de données satellites est devenue facilement disponible.

L'éruption du Mont Pinatubo, en 1991, a procuré un bon laboratoire pour le test des modèles (voir la figure). Non seulement le refroidissement d'environ $0,5^{\circ}$ qui en a résulté a été précisément prévu peu de temps après l'éruption, mais les rétroactions par l'intermédiaire du rayonnement de la vapeur d'eau ou de la dynamique qui étaient incluses dans les modèles ont été quantitativement vérifiées.

Plus d'une douzaine d'équipes à travers le monde développent des modèles climatiques, dont la capacité à simuler le climat présent s'est améliorée significativement sur les vingt dernières années. D'une façon fort intéressante, la moyenne de tous les modèles fait invariablement mieux que chacun des modèles individuellement ce qui montre que les erreurs des simulations sont non biaisées et cela de façon surprenante. Des biais significatifs communs à la plupart des modèles existent cependant, par exemple dans les caractéristiques des précipitations tropicales.

Les modélisateurs du climat sont particulièrement intéressés de tester la variabilité de leurs modèles. Une part de cette variabilité est intrinsèque, mais les modélisateurs étudient aussi la variabilité causée par des changements dans les forçages extérieurs, tels que l'orbite de la Terre où l'activité solaire. Ces études sont compliquées du fait d'observations très incomplètes, de la nature des données de satellite, des incertitudes dans les forçages et d'autres problèmes.

La comparaison de modèles la plus complète qui ait jamais été conduite est actuellement en cours en utilisant les simulations qui ont été effectuées en 2004- 2005 pour le Groupe Intergouvernemental d'Evaluation du Climat. Ces simulations pour le XXI^e siècle et au-delà sont actuellement examinées par des centaines d'équipes indépendantes qui évalueront la robustesse des résultats et aideront à préciser les problèmes persistants.

citer l'influence des conditions climatiques sur El Nino, ou encore les prévisions d'évolution à l'échelle régionale et la manière dont les simulations des événements rares et extrêmes tels que les cyclones et les vagues de chaleur peuvent être validées. De tels problèmes peuvent requérir de meilleures représentations des propriétés turbulentes de l'atmosphère au voisinage de la surface, les effets des tourbillons océaniques ou de la microphysique des nuages et des aérosols. L'incorporation de paramétrisations plus sophistiquées et l'augmentation constante de la résolution qui accompagne l'accroissement des ressources informatiques suggère que ces modèles continueront de s'améliorer. Cependant, beaucoup de résultats, tels que l'effet du réchauffement par l'accroissement des gaz à effet de serre qui ont d'abord été démontrés par les modèles les plus simples, il y a des décennies se sont avérés extrêmement robustes.

Les modèles climatiques sont inégalés dans leur capacité à quantifier des hypothèses qui, autrement resteraient qualitatives et à générer de nouvelles idées qui peuvent être testées contre des observations. Ils sont loin d'être parfaits, mais ils ont réussi à capter et à décrire les aspects fondamentaux de la circulation de l'atmosphère, de l'océan et de la glace de mer et leur variabilité. Ils constituent donc des outils fort utiles pour estimer les conséquences de l'expérience audacieuse que l'humanité est en train de conduire à l'échelle planétaire.

Gavin Schmidt est chercheur au Goddard Institut for Space Studies de la NASA à New York.

HTML document attachment (untitled-2)

To: gschmidt@giss.nasa.gov
Subject: Interview for Mysteries magazine?
Date: 02/02/07 18:49:54

Hello, Gavin.
How are you?

This is Will Romano. I'm a writer for a magazine called "Mysteries". I'm writing a story about the myth of Noah's flood, the archaeological and geological evidence to support it (if any), the people who search for (and who believe exists) a large wooden ark in Eurasian/Asian mountain chains, ancient global weather patterns, etc.

I am wondering if you'd be available for an interview (next week) to speak a bit about prehistoric weather changes, climate changes, and whether a.) any of your climate models supports the possibility that a global flood occurred (where's the evidence?); b.) it is possible that a global flood, or even some smaller localized floods, could have occurred via waterway displacement or rather severe "bad" weather (ie. could it have rained for 40 days and 40 nights and caused "Noah's flood"?). Essentially, I am looking to speak with an expert in paleoclimatology who can discuss whether any of the Biblical accounts are supported by scientific fact or theory.

I'd also like to discuss any pertinent information you may have come across via the Web site, www.RealClimate.org.

My piece will give consideration to a wide range of topics such as William Ryan's/Walter Pitman's "Black Sea" theory (and whether geological evidence supports a catastrophic flood in the appropriate time frame), global flood stories/myths and their connections, and even the assertions of those who are actively searching for an "ark" today.

My work has appeared in a variety of publications, such as New York Post, NY Daily News, Writer's Digest, Military History, Blues Revue, VH-1.com, and others. I am the author of two books, including the recently published *Big Boss Man: The Life and Music of Bluesman Jimmy Reed* (Backbeat/Hal Leonard).

Thanks for your time.

Best,

Will

Will Romano
315 W. Market St.
Long Beach, NY 11561
516-632-5559
WJRY98@aol.com

To: Gavin Schmidt <gschmidt@giass.nasa.gov>

Subject: Re: Friday's Climate@Home meeting

Date: 02/06/07 18:20:08

Gavin:

good to have you chime in. I greatly enjoy your work on the realclimate.org site and watch the site regularly as well as recommend it in my public climate lectures (I give about 1 a month and have done the university, art-house movie theatres, air/space museum, rotary club, church groups, etc.) so I recommend realclimate.org as the climate science blog for up to the minute news.

the CPDN for me was an epiphany in terms of how to think about climate prediction uncertainty and tie it to observation requirements. but many in the U.S. modeling community remain extreme skeptics.

cheers
bruce

>Don, I'd like to second Bruce on this. There is serious science that can
>be done with PPEs and it isn't being covered by the existing CPDN
>effort. I see our contribution to this as a method by which to enhance
>our computing capacity significantly. If a DOE/NSF machine of the
>capacity you mention were available (do you have a timescale?), any such
>PPE use would have to compete against some very worthy projects indeed
>and even hoping for a 1/12 of the years' machine time might be extremely
>optimistic. With a CPDN-like effort, there is no such competition since
>the resources are only available for ensemble runs and not for
>super-high resolution runs etc.

>

>Gavin

>

>On Tue, 2007-02-06 at 13:45, Bruce A. Wielicki wrote:

>> Don:

>>

>> I agree that a huge rapid effort is a bad approach. I also agree that
>> a clear science focus is critical. Instead, after some up front
>> thought and advice from those who have done it, we should engage a
>> modest activity to complement the UK efforts and primarily to deal
>> with two science issues:

>>

>> a) are the UK results using the UKMO model robust to very different
>> model structures? this seems to be a consistent scientific criticism
>> that needs to be addressed. an example might be Model E, but it need
>> not be a huge effort to test: a few hundred to a few thousand model
>> runs might be enough.

>>

>> b) can the PPE approach help the climate community develop more
>> rigorous climate observing system requirements? this is a science
>> question relevant to nasa observing priorities. we currently have no
>> climate OSSE (Observing System Simulation Experiment) capability.

>> this is more about analyzing what PPE experiments can tell us is key
>> in testing the accuracy of climate prediction. the primary reason we
>> might want to do our own is to allow control of the model output
>> metrics being tested. but this could wait while we build up the

>>

>> If DOE, NOAA, or NCAR are already planning on trying item a), then
>> nasa efforts might be better focused on b).

>>

>> In a minimalist world with tight budgets, we could look first for
>> early success in analysis of the UKMO and Oxford results to attack
>> item b): Yong Hu is already being funded on his NRA proposal to do
>> this, and early results look encouraging.

>>

>> I'm assuming the "Toy" comment comes from how far you push this into
>> the education arena, and basically making it a SimEarth sort of
>> climate exploration. In that venue it would be an educational toy. A
>> useful educational tool, but still would "look" like a toy.

>>

>> We only grow an initial modest effort if it proves successful and
>> useful. And the education aspect is much less critical than the
>> science rationale.

>>

>> My own view remains that the PPE approach is the ONLY way we will ever
>> get observation requirements and priorities that are based in a
>> physical hypothesis: a fully coupled climate model. Everything else
>> we use is individual intuition, committee intuition, politics, or more
>> commonly the hot button of the day. It is basically analogous to a
>> random walk to climate change observing requirements. Not very
>> rigorous and won't stand scrutiny. When we walk into OMB or decide
>> how much is needed in a decadal study we do one of three things:

> >

>> a) we need it all (bretherton report in the 1980s that started EOS,
>> which was then downsized 3 separate times by roughly 30% each time)

>>

>> b) we have a budget reduction of X%, so change what you will do by X%
>> (EOS rebaselines in 1990s)

>>

>> c) we think we can convince congress to give us X dollars, so we ask
>> for X (recent nrc decadal study)

>>

>> These are weak arguments to base a global climate observing system
>> on. Thats why they don't hold up over time. There are no
>> requirements, only wishes and votes.

>>

>> In the simplest sense, there are 1000s of climate metrics (model vs
>> observations) in the climate journals. They almost all invalidate the
>> models. But which ones matter most for predicting a given climate
>> change variable? When is a model accurate enough? These are
>> fundamental climate prediction science questions that the PPE approach
>> may well be the only way to address. It was one of three major
>> approaches to evaluation of climate prediction uncertainty used in the
>> new IPCC AR4 draft. Yet little to nothing on this appears to be going
>> on in the U.S. climate model research community. Have I missed it?

>>

>> If there is a better or more promising approach to develop a Climate
>> OSSE capability for NASA and NOAA long term climate observations, I'd
>> love to hear/read about it. Climate models are our hypothesis on the
>> physics of the climate system. Embedded in them and their uncertain
>> physics are the hypothesis tests we need to make. Whether PPE or some
>> other approach, we need to tie our observing system requirements to
>> physical hypothesis not to intuition.

>>

>> slow and prototype/test in small chunks. Build it successful.
>> Eliminate if not. Save education for later if funding is so tight you
>> can barely start the science. Or NSF might want to tackle that one.

>> cheers
>> bruce

>> At 10:23 AM -0500 2/6/07, Donald Anderson wrote:

>> > I am getting significant feedback that looks at all this as a bit of
>> > a toy, and of limited value to climate science.
>> > In particular, why bother with such a complex effort to address
>> > science questions, when we can get to, e.g. DOE machines and get
>> > thousands of processors to do an arbitrary number of ensemble
>> > members?

>> > DOE and NSF have plans for, or have on the floor, machines with in
>> > excess of 150,000 processors. Accessing any one of those for say a
>> > month might be a much simpler solution.

>> > The discussion in this email is not about E&O at all, just do we
>> > need it for the science?
>> > Unless we can make a compelling case for a scientific advantage to
>> > the overhead required to 'manage' thousands of as available
>> > machines, this won't happen in MAP.

>> > My 'global' interest in this remains, but I'm not sure we have a
>> > science driver yet for proceeding with this approach.
>> > I am fairly confident that at least episodically I can find
>> > resources at the 100,000+ processor level to attack a specific short
>> > list of high priority questions.

>> > Note that for the MAP Science Team Meeting, CPDN is being removed
>> > from the agenda. The original idea has been submerged by the
>> > changed due date of Senior Review proposals.
>> > So bringing this together before the meeting is not necessary, or
>> > possible.

>> > What Mark has done helps us think about some of the practical
>> > issues, and SIVO should continue to assist in getting us to a
>> > preliminary strawman, but anything beyond that may require setting
>> > up a workshop to specifically discuss the science return on
>> > investment with this approach. And a strawman design/estimate would
>> > help frame that discussion.

>> > Don

>> > On 2/5/07 6:03 PM, "Bruce A. Wielicki" <b.a.wielicki@nasa.gov>
>> > wrote:

>> > Mike and Mark:

>> > I'm also glad to see the level of interest. I like Mark's
>> > initial schematic to help in the thought process. Some
>> > points I'll chime in from our discussions last fall with the
>> > Oxford climateprediction.net group:

>> > I The major challenges

>> > a) quality control of the submitted runs (weeding out
>> > things that went wrong)

>> > b) data storage of the results: the logistics blue and
>> > pink box in the middle marked
>> > "primary server"

>> > II The major things that helped

>> > a) BOINC (they initially did their own but decided in
>> > the end it was better to use BOINC)

>> > III The challenges clarified by the new schematic

>> > a) What is the process we use to define the major
>> > experiments?

>> > b) What is the process we use to define the output data
>> > saved from the model runs?

>> > this will to a great degree focus and limit the
>> > studies and visualization that can be

>> > done. For example, my own interests in using
>> > this as a tool to develop objective

>> > Climate OSSEs: analysis to prioritize climate
>> > observations and to set their

>> > requirements (variable, time/space scales,
>> > absolute accuracy, stability)

>> > c) the data storage in the primary server (central box)
>> > will be driven by answers to
>> > questions a) and b).

>> > cheers
>> > bruce

>> > At 8:16 AM -0500 2/5/07, Michael Seablom wrote:
>> > Mark,

>> > Thanks for your enthusiastic response... John
>> > Dorband is going to be the point of contact at this
>> > end regarding the use of the BOINC infrastructure,
>> > and also setting up a ModelE prototype. I say that
>> > with the assumption we'll use BOINC, I consider it a
>> > likely option but John is going to investigate other
>> > options, as you suggest, with cost-effectiveness in
>> > mind. You also raise some good questions regarding
>> > the management of the project, the proposal process,
>> > data distribution, etc., that I'm assuming NASA
>> > Headquarters will attend. Setting up the prototype
>> > infrastructure, I believe, can and should go forward
>> > as those questions are addressed.

>> > Best,
>> > Mike

Michael Seabloom

Head, Software Integration & Visualization Office,
Code 610.3
NASA Goddard Space Flight Center

Office/ Secretary: +1-301-286-8580
eFax: +1-202-318-2386

On Feb 4, 2007, at 19:26 , Mark Chandler wrote:
Hi all,

We've spent some time this week considering the design of a Climate@Home, which would produce PPE modeling capabilities (perturbed physical ensembles, as discussed in Bruce Wielicki's email of 11/26/06) similar to those of climateprediction.net. Don Anderson's charge was that we, additionally, consider ways to improve on the CPDN design by creating a system that would allow users to design, create and run their own experiments. In this way the huge numbers of CPU contributors could really participate and learn from the experiments that they were running on their computers. Such contributor/participants fall into (at least) three categories: 1) individuals who simply want to learn more about climate modeling and climate science by running experiments of their own design on their own computers, 2) the community of scientists who would propose experiments to use the distributed network (probably via a formal proposal and review process handled by MAP) and 3) educational institutions seeking to participate in large experiments while also providing training and education to students and the general public (schools, museums, etc.).

I've included a schematic design of such a network, which is a straw man for comment and discussion. "Model E" and "EdGCM" are labeled explicitly, but they are simply representative of the two sides of the same software coin - one which provides the scientific modeling

>> > with the option to interact
>> > with climate models, and learn about climate
>> > science topics. The BOINC
>> > software layer is labeled as well, since it
>> > seems logical that we
>> > would collaborate with CPDN to leverage what
>> > they've already learned
>> > about how to create the distributed network
>> > to run PPE climate model
>> > experiments. In about two weeks, we are
>> > going to begin setting up a
>> > small BOINC network using our local EdGCM
>> > client/server network, just
>> > so we understand what we're dealing with.
>> > However, Mike Seablom, David
>> > Herring and crew at GSFC, together with
>> > Gavin at GISS, will need to
>> > discuss if BOINC is the most logical option
>> > for distributed computing
>> > with Model E. I've already heard from David
>> > Rind regarding numerous
>> > possible PPE experiments that would be of
>> > interest, including PPEs
>> > related to aerosol, sea ice, ocean and cloud
>> > parameterizations.
>> > Certainly Gavin and Jim will have ideas and
>> > priorities as well, and
>> > they can all be discussed at GISS model
>> > meetings. Ultimately it would
>> > be a group of scientists at GISS, GSFC,
>> > Langley etc. who would design
>> > and approve the initial experiments.
>> > However, Don indicated that the
>> > goal is to design a Climate@Home network that
> > > could eventually be made
>> > available to others as well. I believe the
> > > procedure for this type of
>> > "availability" would be through
>> > Announcements of Opportunity (AO),
>> > which could be used to direct experiments
>> > toward specific goals.

>> > The schematic diagram also makes note of the
>> > fact that some
>> > Institutional Clients, such as universities
>> > and other "big-hardware"
>> > participants, probably have enough
>> > distributed resources of their own
>> > that they may wish to design experiments or
>> > educational exercises (for
>> > courses) that utilize "EdGCM-like"
>> > capabilities to create and run
>> > simulations, without necessarily vetting
>> > everything through a

>> > proposal/review process. In the EdGCM
>> > project we've certainly had
>> > requests for such a capability from large
>> > universities, but haven't

often have their own
clusters that they want students and
professors to be able to take
advantage of and it's preferable that such
clients use their own
resources when possible, as opposed to
having to vet every experiment
idea through a central system. The central
system and the associated
large distributed network would then be
reserved for A0-level
experiments vetted through NASA MAP.

Finally, all participants, whether they be
Individual Clients,
Institutional Clients, or Proposing
Scientists, would have access to
their own results (thus, the two way arrows
everywhere on the
diagram). However, one last group of
potential users are those who did
not participate in experiment design,
creation or running, but would
like to view and analyze the results. You'll
see that the diagram has
two sets of participants labeled "Scientific
Community". The first
group consists of users who proposed
experiments, probably in response
to an Announcement of Opportunity, to run on
the Climate@Home
distributed network. The other group, which
we haven't discussed,
would be those users who request access to
the results of experiments
that they themselves didn't design or
create. How would such requests
be handled? Would the results be openly
available to the community?
Would they be available only to those who
wrote formal proposals to
use the data? Would they be available to
educators in a form that they
could use? Would there be a waiting period
for others while the chief
scientists on a project had "first crack" at
the results? I suppose
NASA has formal standards in place for such
requests, science space
missions must have to deal with this all the
time, but it is an issue
to consider for the Climate@Home project as
well.

Feedback and discussion are welcome and
please let me know if I've
left people off this mailing list that
should be included.

>> > regards, Mark

>> >

>> > p.s. I included this email in the pdf that I

>> > attached since it

>> > contains a lot of explanation of the

>> > diagram. I also attached a second

>> > pdf showing a schematic of the BOINC network

>> > layer.

>> > ----

>> > Mark A. Chandler

>> > Columbia University - CCSR/GISS

>> > 2880 Broadway, New York, NY 10025

>> > phone: NY (212) 678-5644

>> > WI (608) 270-9645

>> > <Climate@Home_Schematic.pdf>

>> > <BOINClayer.pdf>

>> > _____

>> > Don Anderson

>> > 3G84

>> > Modeling, Analysis and Prediction (MAP)

>> > Earth Science Division

>> > Science Mission Directorate

>> > NASA HQ

>> > Washington, DC, 20546-0001

>> > 202-358-1432 Fax: x2770

>> > email: Donald.Anderson-1@nasa.gov

>> --

>> Bruce A. Wielicki

>> Mail Stop 420

>> NASA Langley Research Center

>> Hampton, VA 23681-2199

>>

>> Phone: (757) 864-5683

>> FAX: (757) 864-7996

--

Bruce A. Wielicki

Mail Stop 420

NASA Langley Research Center

Hampton, VA 23681-2199

Phone: (757) 864-5683

FAX: (757) 864-7996

To: Benzil, Deborah <DEBORAH_BENZIL@>, Danienncn21@
Subject: Re: Letter attached
Date: 03/13/07 22:23:07

Thanks for your letter. This is a subject we have worked on for a while, and you might like to read some of other papers on the subject. This is for 'popular' science purposes:
http://www.giss.nasa.gov/research/briefs/legrande_01/

these relate to some of the recent research in the area:
<http://www.realclimate.org/index.php/archives/2005/05/gulf-stream-slowdown/>
<http://www.realclimate.org/index.php/archives/2005/11/decrease-in-atlantic-circulation/>
<http://www.realclimate.org/index.php/archives/2006/10/ocean-circulation-new-evidence-yes-slow>

and this is more technical:
http://pubs.giss.nasa.gov/abstracts/2006/LeGrande_etal.html

Feel free to come back and ask questions if you like,

Gavin

On Tue, 2007-03-13 at 16:54, Benzil, Deborah wrote:

- > Dr. Schmidt,
- > I have attached a letter from my son, sent from this site since I know that many educational and governmental organizations do not allow mail from "aol". I hope you will have time to read and respond. His e-mail address is included in the correspondence or you can write through this site if necessary.
- >
- > Deborah L. Benzil, M.D.
- > Clinical Associate Professor
- > Department of Neurosurgery

To: gschmidt@giss.nasa.gov
Subject: Re: Letter attached
Date: 03/14/07 13:27:40

Dear Gavin,

Thank you very much for responding to my email I have started to read the technical article, due to the fact that I had previously read them and I found them very informative. Actually I found you through realclimate.org and then from there I did a literature search on other articles that you have written I found a lot before emailing you. I am very interested in this field of study, because I feel that unlike some other problems this effects the whole world and could end up in a catastrophe. I was wondering about the computer models, how in detail do they generally get, and how long do they take to run. As I was reading "Effects of Glacial Meltwater on the GISS Coupled Atmosphere-Ocean Model Part 1: North Atlantic Deep Water Response," I noticed that you said the models were not very detailed, so do they get more in detailed or not?

Thank you very much for your time.

Daniel Finch

-----Original Message-----

From: gschmidt@giss.nasa.gov
To: DEBORAH_BENZIL@ [REDACTED]; Danielfinch21 [REDACTED]
Sent: Tue, 13 Mar 2007 10:3 PM
Subject: Re: Letter attached

Thanks for your letter. This is a subject we have worked on for a while, and you might like to read some of other papers on the subject. This is for 'popular' science purposes:

http://www.giss.nasa.gov/research/briefs/legrande_01/

these relate to some of the recent research in the area:

<http://www.realclimate.org/index.php/archives/2005/05/gulf-stream-slowdown/>

<http://www.realclimate.org/index.php/archives/2005/11/decrease-in-atlantic-circulation/>

<http://www.realclimate.org/index.php/archives/2006/10/ocean-circulation-new-evidence-yes-slow/>

and this is more technical:

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Feel free to come back and ask questions if you like,

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>

> Deborah L. Benzil, M.D.

> Clinical Associate Professor

> Department of Neurosurgery

AOL now offers free email to everyone. Find out more about what's free from AOL at AOL.com.

To: gregory murphy <gregorymurp@>
Subject: Re: climate modelling
Date: 04/16/07 20:39:03

Sure. There are pop-sci pieces:

http://pubs.giss.nasa.gov/abstracts/2007/Schmidt_1.html
<http://www.realclimate.org/index.php/archives/2005/01/is-climate-modelling-science/>
(other stuff on realclimate can be found from:
<http://www.realclimate.org/index.php/archives/2004/12/index/#ClimateModelling>

or stuff in the technical literature:

http://pubs.giss.nasa.gov/abstracts/2006/Schmidt_etal_1.html
http://pubs.giss.nasa.gov/abstracts/2004/Schmidt_etal_3.html
http://pubs.giss.nasa.gov/abstracts/inpress/Hansen_etal_1.html
http://pubs.giss.nasa.gov/abstracts/inpress/Hansen_etal_3.html

but let me know if there is something specific you'd like to know about.
It might be faster than wading through that lot!

Gavin

On Mon, 2007-04-16 at 20:25, gregory murphy wrote:

> I worked through your posting on Real Climate and was wondering if you
> have an articles or briefing papers on the physics of climate
> modeling and i was wondering about abrupt climate change and how the
> models differ in the two very different cases. I am still trying to
> put the whole climate change debate in order in my head and I say
> that your little exercise was a great help. thanks for any and all
> help
> in this matter. I have a real quest to learn and my background is
> nuclear power so I would like to know how to tell that I am being lied
> to
> by the press like fox news and how i can show that they are full of
> bull. thanks greg

To: Max Berkelhammer <berkelha@>
Subject: Re: Isotope Inquiry
Date: 04/27/07 13:04:40

Hi Max, I haven't looked particularly at multidecadal or centennial variations in $\delta^{18}O_{precip}$, though I suppose we could. However, I would be extremely doubtful that you'll see anything like 6 permil shifts in control run. Our simulations with complete collapses of the NADW only produce around -2 permil in the southern US (though you do get bigger changes in the North Atlantic). Our 8.2kyr simulations showed a few tenths of a permil change in that region. ENSO variations are possibly on the order of a permil or so.

Thus I would be looking for reasons why the cellulose is amplifying some signal, drought perhaps? or for a possible non-climatic issue.

Hope that helps,

Gavin

On Fri, 2007-04-27 at 01:30, Max Berkelhammer wrote:

> Dear Dr. Schmidt,
> I am a PhD student in Lowell Stott's paleoclimate lab at USC and was
> hoping I could bounce a quick question off you. I have been generating
> an oxygen-18 time series from long-lived tree cellulose in the western
> United States. The data seems to indicate that there are major
> low-frequency trends in the isotopic composition of the precipitation
> in the region. I have followed your publications on modeling isotopes
> in precipitation and was curious if you observe centennial-scale
> patterns in the isotopic ratios in precipitation? Some of the
> excursions are on the order of 6-8 per mil, which makes me think it is
> not exclusively a function of changes in source water.

> Thanks. As a side note, I am an avid reader of your writings on
> realclimate.org and really appreciate your efforts to address a vital
> need.

> Best,
> Max

> Max Berkelhammer
> University of Southern California
> Department of Earth Sciences
> 3651 Trousdale Pkwy.
> Los Angeles, CA 90089
> berkelha@
> <http://earth.usc.edu/~berkelha>
>

To: gschmidt@giss.nasa.gov
Subject: Open access to IPCC AR4 climate model data
Date: 05/23/07 13:49:22

Hi Gavin,

First of all, allow me to introduce myself -- I'm a software engineer and climate data gadfly at NOAA/PMEL in Seattle. I regularly read (and enjoy!) your RealClimate blog.

I am interested in presenting the data from model runs for the IPCC AR4 in a way that would provide easier access to non-specialists (a reduced functionality prototype of this is available at <http://dappertest.pmel.noaa.gov/dchart/>). I recently discovered, to my dismay, that with the exception of GFDL, access to data from climate model runs for the IPCC AR4 isn't publicly available. When I applied for access, I was told that redistribution of the data was prohibited. This prohibition is apparently comprehensive and includes the data from GFDL, NCAR, and NASA, even though that data should be in the public domain. I believe this policy sends the wrong message to the public and could provide fodder for climate change skeptics, but that's a discussion for another day...

Would it be possible for you to make the model data you submitted to PCMDI publicly available on a NASA Web server? My software only requires HTTP access to the netCDF files, so my only requirement would be that the files are available on a public Web server. The software only downloads the data that is requested (not the entire dataset) so the server load should be minimal.

Cheers, Joe Sirott

P.S. BTW, this was inspired by the RealClimate entry on the paper by Gabe Vecchi. I worked a bit with Gabe a few years ago and was curious about the data behind his paper and that lead me to the closed data archive at PCMDI...

To: Holli Riebeek <hriebeek@climate.gsfc.nasa.gov>

Subject: Re: Link to "physics of climate modeling"?

Date: 06/14/07 15:21:47

sure, should be no problem.

gavin

On Thu, 2007-06-14 at 15:10, Holli Riebeek wrote:

> Hi Gavin,

> Would you mind if I included a link to your article, The Physics of Climate
> Modeling, as a reference in a global warming question and answer page on
> the Earth Observatory? As you know, we published the global warming fact
> sheet in May. We asked readers to send us questions, and we're following up
> with a global warming Q&A. One of the questions is "What if we're wrong
> about the severity of global warming?" (My response follows, if you're
> interested.) Each response includes a list of references, and since model
> uncertainty is part of this question, I think it would be appropriate to
> include a link to your article at

> http://www.giss.nasa.gov/research/briefs/schmidt_04/. I see that it is
> copyrighted, so I want to make sure it's ok to include the link.

> Thanks,

> Holli

> What if we are wrong about the severity of global warming? What if the
> problem isn't as serious as predicted?

> If models are wrong about the severity of global warming, it is because
> Earth's climate is either more or less sensitive to change than we think it
> is because of climate feedbacks. Feedbacks are events that either limit or
> amplify climate change once an external factor like a rise in greenhouse
> gases initiates change. (See [[link to feedback question](#)] for a more
> complete discussion.)

> Some argue that there may be as-yet-unidentified feedbacks in Earth's
> climate system that will regulate global warming (negative feedbacks). If
> this is the case, they contend, then we should not waste money trying to
> mitigate global warming. However, most scientists believe that if there are
> hidden feedbacks, they are just as likely to amplify warming (positive
> feedbacks). In other words, there is just as much chance that the models
> are underestimating the severity of future warming as they are
> overestimating warming.

> Given the potentially catastrophic effects of global warming, uncertainty
> is not a good reason to delay action. If we do reduce emissions and climate
> change turns out to be less serious than predicted, we still benefit from
> our efforts. By switching to renewable energy sources like solar and wind,
> we can reduce our dependence on oil (a limited resource) and improve our
> air quality.

> IPCC, 2007: Summary for
> Policymakers. In: Climate Change 2007: Mitigation of Climate Change
> Group III to the Fourth Assessment Report of the Intergovernmental Panel on
> Climate Change. Cambridge University Press, Cambridge, United Kingdom and
> New York, NY, USA.

> Pierrehumbert, R. (2005). <a href="

> <http://www.realclimate.org/index.php?p=229>>Natural Variability and Climate

>
> Schmidt, G. (2007, January). <a
> href="http://www.giss.nasa.gov/research/briefs/schmidt_04/">The Physics of
> Climate Modeling. Physics Today, 72. Also available from the Goddard
> Institute of Space Studies. Accessed June 14, 2007.
>
>
>
> Holli Riebeek, SSAI
> Outreach coordinator, MODIS Rapid Response Project
> Science writer, NASA's Earth Observatory
> http://rapidfire.sci.gsfc.nasa.gov
> http://earthobservatory.nasa.gov
> E-mail: hriebeek@climate.gsfc.nasa.gov
> Phone: 301-614-5753
>
>
>

To: Phil Jones <p.jones@uea.ac.uk>

Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>, d.lister@uea.ac.uk, Reto Ruedy <rruedy@giss.nasa.gov>, Makiko Sato <makis@giss.nasa.gov>

Subject: Re: Recent events

Date: 08/15/07 10:12:05

Hi Phil,

Thanks, this is very helpful. Glad that you are keeping on top of these things -- we can only give a small fraction of our time to this topic, so it is good that someone keeps on top of it. By the time you are ready to retire global warming may be large enough that it doesn't require that level of expertise and detail. I am especially interested in your comments about NOAA SSTs -- they seemed just a hair cool to me.

Jim

On 8/15/07, **Phil Jones** <p.jones@uea.ac.uk> wrote:

> Jim, Gavin,

Your recent finding of NCDC/GHCN not continuing to adjust records in real time and its impacts on blog sites, has alerted me to inform you of a few things we've been doing over the past year. Don't pass any of this on via Real Climate or whatever. Eventually, we will get around to documenting all we've been doing.

1. You may have noticed that Canada has changed loads of its WMO IDs. We've been in contact with Lucie Vincent there who's also been doing some homogeneity work (which is good by the way). As a result of this we are applying adjustments in real time to about 40 stations (mainly in the east of the country) in order to use her long adjusted series. Why she adjusted records to an earlier period still isn't clear to me. We are also getting on top of their station number changes, which appear related to automation - and giving the new AWSs a new number.

By the way the AES web site enables you to get their real time data, but there is no mention there of another page which gives the homogeneous series!

This is all less important with your method of combination. Ours requires normals.

2. We're getting all Australian data in real time direct from the National Climate Centre in Melbourne.

3. We've got all the long NZ series they have homogenized.

~~Problems with both Australia and NZ associating these with WMO IDs we had.~~

~~Why it's always the English speaking countries is odd? Maybe this is because we can find out/understand more easily what they're doing!~~

4. My biggest worry is China. CMA don't measure at airports, and they keep moving suburban locations a few more miles out as the cities expand. I was there a month ago to give some talks. I've sent them all the CRU data for China, in the hope that they will reciprocate at some point and send me their adjusted data (for site moves, but not urban influences). They are doing some reasonable work, but not seeing the big picture...

Other issues:

1. I reviewed a paper by NCDC (Smith/Reynolds/Peterson) recently. It was OK, but when it comes out it will raise the whole debate again. SSTs are being increasingly measured by buoys (drifting and fixed) and they now dominate over the ships. It seems they are about 0.1-0.2C cooler over the ships. So NCDC will be increasing global temps from about 2000 onwards.

2. SSTs are now coming in for the areas losing Arctic sea ice. The normals we have for these are -1.8C which is completely wrong. Shortish time series are composed of entirely positive anomalies. Maybe this is true, but it probably shouldn't be as much as it is. This problem will get worse as the sea ice continues to go. Your use of land only data shouldn't have the problem.

The SST issues highlight that it is the biases (bucket/intakes and urbanization) that are important as they are potentially pervasive. Individual station homogeneity issues cancel as sites are all affected differently. Getting this right has hardly any effect (none in fact) on the large-scale averages. Might affect smaller regions, and it's good to get as many right as possible, as the deniers will claim if one is wrong the whole lot is wrong. The law of large numbers seems to be totally forgotten by those collecting pictures of sitings across the US. Still it gives them something to do...

Cheers
Phil

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

Reply-to: rruedy@giss.nasa.gov

To: James Hansen <jhansen@giss.nasa.gov>

Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>, Reto Ruedy <cdrar@giss.nasa.gov>, Makiko Sato <makis@giss.nasa.gov>

Subject: Re: Town Hall Story on NASA blocking McIntyre access

Date: 08/17/07 19:28:04

I understand, that was just meant as a suggestion to bring up on Gavin's RealClimate site, if he needs to counter requests for our "fixing" code.

Reto

On Fri, 2007-08-17 at 19:06 -0400, James Hansen wrote:

> Technical arguments with a jackass or a jester, which most observers
> not wanting to understand the details, can appear to lower one to a
> comparable level. Better not argue with him about whether we fix
> data; we do an urban adjustment, for example. Jim

>
> On 8/17/07, Reto Ruedy <rruedy@giss.nasa.gov> wrote:

> TOBS does not have the station history adjustment (SHAP) -
> FILIN has it
> and is the last stage before their urban adjustment. I can run
> with or
> without the filled-in data (filling in added .05C/century to
> the US mean
> trend in our analysis).

>
> Once the new USHCN data are reformatted, it's just a question
> of what to
> do with years 2006 and 2007. Otherwise it's simply switching
> an input
> file.

>
> I still think, Steve (in the Town Hall interview below and
> when he talks
> to anybody but us) mixes us up with Tom Karl's group - they
> "fix"
> station data, we don't. If we get this misunderstanding out in
> the open,
> it might die down as well.

>
> Reto

>
> On Fri, 2007-08-17 at 16:23 -0400, Gavin Schmidt wrote:
> > I didn't suggest using their urban adjustment, but that the
> most
> > up-to-date USHCN data may have more in the way of documented
> station
> > adjustments and more data earlier on. The FILIN data do not
> include
> > their urban adjustment as far as I can tell. I get the
> impression from
> > the USHCN web site that you should be able to extract just
> the TOBS

> > corrected data without the FILIN.

> >
> > The point is to make sure that the difference between the
> earlier USHCN

> significant difference to the results. Since any independent
 > replication
 > of the GISS procedure will use the currently available data
 > set (not the
 > one we are using), we should probably be ahead of the game
 > in
 > understanding what impact it has.
 >
 > As is usual in these cases, the smarter of the court jesters
 > have
 > already stopped talking about 1934 and are now pushing the
 > transparency
 > 'meme'. That has a lot more resonance....
 >
 > Gavin
 >
 > On Fri, 2007-08-17 at 16:10, James Hansen wrote:
 > > What is the matter with the way that we do it? Among
 > other things, we
 > > have a more realistic urban adjustment. Changing has
 > various
 > > drawbacks. Jim
 > >
 > > On 8/17/07, Reto Ruedy <rruedy@giss.nasa.gov> wrote:
 > > Jim,
 > >
 > > Gavin suggested some time ago that we should do
 > the analysis
 > > with the
 > > current USHCN.
 > >
 > > I downloaded the "FILIN" USHCN data; the filled-in
 > numbers are
 > > marked.
 > > So I can use or ignore them. I have to write a
 > program anyway
 > > to
 > > reformat this file to the format used by GHCN.
 > This includes
 > > the easy
 > > conversion from F to C, but they also use a
 > different set of
 > > ID-numbers
 > > to characterize the station. So first, I'll have
 > to construct
 > > and check
 > > a conversion table to identify the stations
 > properly.
 > >
 > > Reto
 > >
 > > On Fri, 2007-08-17 at 11:44 -0400, Reto Ruedy
 > wrote:
 > > > What I wrote was true last week - today it says
 > that monthly
 > > data are
 > > > available from 1900-2005. They must have updated

> > > last few
> > > days.
> > >
> > > Reto
> > >
> > > On Fri, 2007-08-17 at 11:35 -0400, Reto Ruedy
wrote:

> > > Jim,
> > >
> > > On the USHCN site it says that the data
available from
> > > their web site go
> > > to 2002. I never downloaded them since the
stage we use is
> > > not stored at
> > > that site - we would have to make a special
request.

> > >
> > > Reto
> > >
> > > On Fri, 2007-08-17 at 11:18 -0400, James
Hansen wrote:

> > > > Mc claims that USHCN data is actually
available

> > > > up-to-date. Is that
> > > > right? Jim

> > > >
> > > > On 8/17/07, lesgiss@verizon.net
<lesgiss@verizon.net>

> > > wrote:

> > > > Good morning:

> > > >
> > > > Here is the Town Hall story entitled

"NASA

> > > Blocked Climate

> > > > Change Blogger
> > > > from Data"...

> > > >
> > > >

> > > > http://www.townhall.com/Columnists/AmandaCarpenter/2007/08/17/nasa_blocked_c

> > > >
> > > >

> > > > [climate_change_blogger_from_data?page=full&comments=true](http://www.townhall.com/Columnists/AmandaCarpenter/2007/08/17/nasa_blocked_climate_change_blogger_from_data?page=full&comments=true)

> > > >
> > > >

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> > >
> > >
> > >
> >
> > Reto Ruedy <rruedy@giss.nasa.gov>
> >
> --
> Reto Ruedy <rruedy@giss.nasa.gov>
>
--
Reto Ruedy <rruedy@giss.nasa.gov>

to: Mike Bauer <mbauer@giss.nasa.gov>

Subject: Re: interesting spot

Date: 08/23/07 12:23:12

<http://www.realclimate.org/index.php/archives/2007/08/musings-about-models/>

On Thu, 2007-08-23 at 12:18, Mike Bauer wrote:

> Gavin,

>

> Thought this might be of interest.

>

> Mike

>

> Statistics and climatology

>

> Gambling on tomorrow

> Aug 16th 2007

> From The Economist print edition

>

> Modelling the Earth's climate mathematically is hard already. Now a
> new difficulty is emerging

>

> "SCIENCE" is a recently coined word. When the Royal Society, the
> world's oldest academy of the discipline, was founded in London in
> 1660, the subject was referred to as natural philosophy. In the 19th
> century, though, nature and philosophy went their separate ways as
> the natural philosophers grew in number, power and influence.

>

> Nevertheless, the link between the fields lingers on in the name of
> one of the Royal Society's journals, Philosophical Transactions. And
> appropriately, the latest edition of that publication, which is
> devoted to the science of climate modelling, is in part a discussion
> of the understanding and misunderstanding of the ideas of one
> particular 18th-century English philosopher, Thomas Bayes.

>

> Bayes was one of two main influences on the early development of
> probability theory and statistics. The other was Blaise Pascal, a
> Frenchman. But, whereas Pascal's ideas are simple and widely
> understood, Bayes's have always been harder to grasp.

>

> Pascal's way of looking at the world was that of the gambler: each
> throw of the dice is independent of the previous one. Bayes's allows
> for the accumulation of experience, and its incorporation into a
> statistical model in the form of prior assumptions that can vary with
> circumstances. A good prior assumption about tomorrow's weather, for
> example, is that it will be similar to today's. Assumptions about the
> weather the day after tomorrow, though, will be modified by what
> actually happens tomorrow.

>

> Psychologically, people tend to be Bayesian—to the extent of often
> making false connections. And that risk of false connection is why
> scientists like Pascal's version of the world. It appears to be

> objective. But when models are built, it is almost impossible to
> avoid including Bayesian-style prior assumptions in them. By failing
> to acknowledge that, model builders risk making serious mistakes.

>

> Assume nothing

> In one sense it is obvious that assumptions will affect outcomes—
> another reason Bayes is not properly acknowledged. That obviousness,
> though, buries deeper subtleties. In one of the papers in
> Philosophical Transactions David Stainforth of Oxford University
> points out a pertinent example.
>
> Climate models have lots of parameters that are represented by
> numbers—for example, how quickly snow crystals fall from clouds, or
> for how long they reside within those clouds. Actually, these are two
> different ways of measuring the same thing, so whether a model uses
> one or the other should make no difference to its predictions. And,
> on a single run, it does not. But models are not given single runs.
> Since the future is uncertain, they are run thousands of times, with
> different values for the parameters, to produce a range of possible
> outcomes. The outcomes are assumed to cluster around the most
> probable version of the future.
>
> The particular range of values chosen for a parameter is an example
> of a Bayesian prior assumption, since it is derived from actual
> experience of how the climate behaves—and may thus be modified in
> the light of experience. But the way you pick the individual values
> to plug into the model can cause trouble.
>
> They might, for example, be assumed to be evenly spaced, say 1,2,3,4.
> But in the example of snow retention, evenly spacing both rate-of-
> fall and rate-of-residence-in-the-clouds values will give different
> distributions of result. That is because the second parameter is
> actually the reciprocal of the first. To make the two match, value
> for value, you would need, in the second case, to count 1, $\frac{1}{2}$, $\frac{1}{3}$,
> $\frac{1}{4}$ —which is not evenly spaced. If you use evenly spaced values
> instead, the two models' outcomes will cluster differently.
>
> Climate models have hundreds of parameters that might somehow be
> related in this sort of way. To be sure you are seeing valid results
> rather than artefacts of the models, you need to take account of all
> the ways that can happen.
>
> That logistical nightmare is only now being addressed, and its
> practical consequences have yet to be worked out. But because of
> their philosophical training in the rigours of Pascal's method, the
> Bayesian bolt-on does not come easily to scientists. As the old saw
> has it, garbage in, garbage out. The difficulty comes when you do not
> know what garbage looks like.
>
>
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> rights reserved.

To: Jay Gulledge <Gulledgej@pewclimate.org>
Subject: RE: Conflicting reports on temp correction
Date: 08/27/07 15:28:58

no. I used the wayback machine, there might be more there.

Gavin

On Mon, 2007-08-27 at 15:27, Jay Gulledge wrote:

> Gavin,
>
> That's proof positive that global warming stopped in 1998!
>
> Thanks for the clarification. Is there any archive of the annual
> analyses so that one could look how it changed over time?
>
> Thanks,
> -jay
>

> -----Original Message-----

> From: Gavin Schmidt [mailto:gschmidt@giss.nasa.gov]
> Sent: Monday, August 27, 2007 3:23 PM
> To: Jay Gulledge
> Subject: RE: Conflicting reports on temp correction
>

> The 'pre-correction' data linked on RC are the ones from 2006. The
> numbers last changed in Jan 2007 (as we assimilated the 2006 data) and
> it was at that point that it changed to having 1998 ahead (as you note
> by 0.01 deg C). There was no fanfare or announcement of any sort.
>

> Gavin
>

> On Mon, 2007-08-27 at 15:13, Jay Gulledge wrote:

> > Ah, I found your link to the pre-correction data! This data set has
> > 1934 and 1998 tied at 1.24 C. The corrected data has 1934 at 1.25 and
> > 1998 at 1.23, but your RealClimate post says the pre-corrected temp
> > for 1934 was 1.23. Now I'm really confused!
> >

> > -jay
> >

> > From: Jay Gulledge
> > Sent: Monday, August 27, 2007 3:07 PM
> > To: Gavin Schmidt [gschmidt@giss.nasa.gov]
> > Subject: Conflicting reports on temp correction
> >

> > Gavin,
> >

> > I'm writing a brief post for the Pew web site about the NASA data
> > correction. Jim Hansen insists that the relative ranking of 1998 and
> > 1934 was not affected by the correction, citing his 2001 paper, which
> > indeed says 1934 was warmer in the GISS analysis. But in your
> > RealClimate post you say the ranking did switch, but then you quote
> > the 2001 paper saying that 1934 was warmer. I was not able to find the

> > pre-correction data to check this for myself, but it seems possible
> > that there were minor tweaks after 2001 that made the two years change
>

> >
> > I know it's foolish to focus on individual years and will say so in my
>
> > post, but I need to know how the data set actually changed.
> >
> > Thanks,
> > -jay
> >
> > Jay Gulledge, Ph.D.
> > Senior Scientist
> > Program Manager for Science & Impacts
> > Pew Center on Global Climate Change
> > 2101 Wilson Blvd, Suite 550
> > Arlington, VA 22201
> >
> > Email: GulledgeJ@pewclimate.org
> > Phone: 703-516-0610
> > Fax: 703-841-1422
> >
>

TO: lesqiss@verizon.net

Subject: Re: FW: Question about Warmest year

Date: 08/27/07 15:24:22

point him to the RC piece and to Jim's Light's out piece. But if he wants to talk to someone he can call me.

Gvain

On Mon, 2007-08-27 at 15:18, lesqiss@verizon.net wrote:

> Hi...would one of you like to reply to him?

>

> Thanks.

>

> Leslie

>

> Original Message:

> -----

> From: Alan Dyer alandyer@telusplanet.net

> Date: Sat, 25 Aug 2007 09:31:21 -0600

> To: Leslie.M.McCarthy@nasa.gov

> Subject: Question about Warmest year

>

>

> Hello Ms. McCarthy,

>

> I'm with the science centre in Calgary, Canada and am looking for

> some authoritative and quotable information on what Goddard

> researchers have measured as the warmest years in the last century.

> The information I have from Goddard press releases such as at:

>

> http://www.nasa.gov/centers/goddard/news/topstory/2006/2006_warm.html

>

> is that 2005, 1998, 2002, 2003 and 2006 are the warmest years of the
> last 100 years.

>

> However, a columnist for a local newspaper in an August 24 editorial

> denying global warming and Arctic ice reduction quotes Goddard

> research as saying this:

>

> -----

> NASA's Goddard Institute for Space Studies had to back down from its

> previous claim that 1998 was the U.S.'s hottest year on record. That

> distinction now goes to 1934.

>

> Indeed, four of the hottest years on record are in the 1930s and

> polar bears survived just fine.

>

> -----

>

> Before I counter with any rebuttal or letter to the editor I wanted

> to check what might be the source of that "fact" attributed to

> Goddard research and how the writer might have misconstrued some

> statement out of Goddard. I'm looking for a quote I can include in my

> rebuttal and even a reference to some published paper that provides

> an authoritative answer to the question of "what were the warmest

> years."

>

>
>
>
> Alan Dyer
> Producer and Astronomer
> TELUS World of Science
> Calgary, Canada
>
>
> PS: I have taken the liberty of sending similar inquiries directly to
> Mark Schoeberl at GSFC and James Hansen at GISS. Thanks!

>
> -----
> -----
> Alan Dyer
> P.O. Box 1436, Stn. M
> Calgary, Alberta
> Canada T2P 2L6
>
> Ph.: (403) 734-3155 (home)
> Ph.: (403) 616-6784 (cell)
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>
>
>
> -----
> mail2web.com What can On Demand Business Solutions do for you?
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To: gschmidt@giss.nasa.gov

Cc: scicommconsortium@gmail.com

Subject: Science Communication Consortium: Invitation to Speak

Date: 09/13/07 14:38:23

Hello Gavin,

I am not sure if you remember me, but we spoke for a while at the "Framing Science" lecture (Mooney & Nisbet) reception and then again briefly at Chris Mooney's book party. The reason I am contacting you is that two other graduate students (Liz and Kate...whom I think you have met as well) have recently teamed up with NYAS to host a lecture series on science communication. The motivation for the Science Communication Consortium sprung from the after-lecture discussions that were had once the three of us left the "Framing Science" talk. You can find more information on the series by going to our [website](#). Basically, we feel that it is important to raise scientists' awareness of the importance of communicating their work to non-scientists. We believe that if there was more of a dialog between the general public, policy makers, and scientists, it would be harder for science to be framed by those special interest groups that try and persuade the public using scientific terminology for personal and/or financial gain.

Our November 15th session will be especially interesting and it is the panel that we are very much hoping you will join. The topic of that panel is 'how scientists can effectively communicate their research to those who might consider that research controversial'.

Many scientific issues, such as evolution, global warming, and embryonic stem cell research, are often used for political purposes or framed to support a specific ideology, and have been dubbed by the press as "hot topic" or contentious areas of research. We believe that other scientists, whether working in these or related fields, should be aware of how these issues can impact specific perceptions of these research topics, general public support for science, and subsequent policy decisions. With your work on climate change and your efforts to communicate climate research through your blog "RealClimate", we would be honored to have you participate in the panel.

I've attached a letter outlining the scope of our lecture series. Please do contact me for further information and with any questions you might have. I'll look forward to hearing from you.

Sincerely,

Katie Abele

scicommconsortium@gmail.com

Word document attachment (SciComm_Speaker_Invitation Schmidt.doc)

To: Grant Foster <grant.foster@[REDACTED]>
Cc: jdannan@jamstec.go.jp, gschmidt@giss.nasa.gov

Subject: RE: comment to jgr

Date: 09/24/07 13:24:14

hello grant; copy to your gavin and james:

that would be great; thx;

If I was misled by the statistics, it is important to find this out.

-steve

At 9:05 AM -0700 9/24/07, Grant Foster wrote:

>Dear Dr. Schwartz, How do you do. I'll be happy to help you get any
>data you want, and answer any questions you have. Gavin (who speaks
>highly of you) can get you the 5 runs of GISS modelE, and James Annan can
>get you the data used from other models (from CMIP3). You can get his
>email from the preprint, and I've already emailed him suggesting that we
>get all the data together to send to you. Sincerely,
>Grant

>

>

>From: stephen e. schwartz [mailto:ses@bnl.gov]

>Sent: Fri 9/21/2007 7:32 AM

>To: Grant Foster

>Subject: comment to jgr

>

>

>

>hello grant foster

>

>First, in response to my request Gavin Schmidt revealed to me that you had
>authored the piece by [REDACTED] on [REDACTED]. I had wanted to
>thank you for the even handed tone of that piece. I infer that there is a
>lot of speculation on the web as to the identity [REDACTED], so I shall keep
>this confidential.

>

>James Annan has made preprint copy of your comment available to me. I am
>impressed by the firepower that you all brought to the comment.

>

>I had written gavin that I noted among other things that you were able to
>apply my analysis (at least of the autocorrelation) to the output of the 14
>models over the twentieth century (\pm). I do not have access to those time
>series in GMST; are they available somewhere? And for that matter is the
>change in ocean heat content available for those models? I see that some
>models tabulate thermosteric sea level rise (which is closely correlated
>with change in ocean heat content). Perhaps you could point me to the heat
>content data if they are tabulated. I do think that

>

>I put this request to gavin last night and I don't want to have you both
>responding to this request if there is much effort involved, so perhaps
>you might want to touch base with gavin.

>

>I may have some more questions or requests as I proceed through your comment.

>

>
>steve schwartz
>
>
>- - - - -
>Stephen E. Schwartz, Senior Scientist
><mailto:ses@bnl.gov>mailto:ses@bnl.gov
>Atmospheric Sciences Division Phone: (631) 344-3100
>Brookhaven National Laboratory Fax: (631) 344-2887
>Bldg. 815E (75 Rutherford Dr.) Admin. Asst: (631) 344-3275
>PO Box 5000 Upton NY 11973-5000
>
> Home Page:
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>
> DOE Atmospheric Science Program:
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Stephen E. Schwartz, Senior Scientist mailto:ses@bnl.gov
Atmospheric Sciences Division Phone: (631) 344-3100
Brookhaven National Laboratory Fax: (631) 344-2887
Bldg. 815E (75 Rutherford Dr.) Admin. Asst: (631) 344-3275
PO Box 5000 Upton NY 11973-5000

Home Page: http://www.ecd.bnl.gov/steve
Admin Asst: Nancy Warren mailto:nwarren@bnl.gov

DOE Atmospheric Science Program: http://www.asp.bnl.gov
- - - - -

To: Drew Shindell <dshindell@giss.nasa.gov>

Subject: Re: Ozone

Date: 09/27/07 13:00:40

attached: the nature commentary, the pope et al paper and the obs paper

thanks

gavin

On Thu, 2007-09-27 at 11:39, Drew Shindell wrote:

> Hi Gavin,

> I have a little time today (or at least don't want to spend the
> whole day on the things I should be doing), so if you were willing to
> take 5 minutes to send me the Nature new story and the paper on the
> observations of ClOOCl that we looked at yesterday (JGR?), I'll write
> a short piece for RealClimate on this. Sorry to ask, but I can't get
> to those sites from home. If not, maybe I can do this tomorrow from
> GISS.

>

> Thanks,

> Drew

PDF document attachment (pope.et.al07.pdf)

PDF document attachment (449382a.pdf)

PDF document attachment (2003JD003811.pdf)

Reply-to: valerie.masson@lsce.ipsl.fr

To: Gavin Schmidt <gschmidt@giss.nasa.gov>

Subject: (pas de sujet)

Date: 10/03/07 12:03:14

Hi Gavin

I hope that you are doing well!

Have you ever considered implementing tritium in the coupled GISS model? I have just been discussing with Philippe Jean Baptiste here. He has a good knowledge of inventories and evolution of tritium in various components in the system. We were discussing moisture advection in Antarctica (cf our common submitted paper) and he thought that it could be very valuable to use all the data to test the reservoir time constant of coupled GCMs (esp. stratosphere and upper ocean) using the wealth of available data.

You had also asked me some information for realclimate and I must said that I have been very busy and unable to provide anything until now. Do you have specific suggestions?

Valérie.

--

<http://www-lsce.cea.fr/Pisp/24/valerie.masson-delmotte.html>

To: Grant Foster <[REDACTED]>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Michael E. Mann <mann@psu.edu>

Subject: [Fwd: 2007JD009373 (Editor - John Austin): Decision Letter]

Date: 11/12/07 17:09:56

Well well. Here are the reviews. It looks like there is no problem overall, but the disparity of the reviews is quite remarkable.

It's rather reminiscent of my attempts at commenting in GRL, but the editor has this time taken the sensible viewpoint of [REDACTED].

At a glance it is not clear how to take account of [REDACTED] of Ref1, [REDACTED].

Ref2 [REDACTED], of course - and he [REDACTED].

James

--

James D Annan jdannan@jamstec.go.jp Tel: +81-45-778-5618 (Fax 5707)
Frontier Research Centre for Global Change, JAMSTEC
3173-25 Showamachi, Kanazawa-ku, Yokohama City, Kanagawa, 236-0001 Japan
<http://www.jamstec.go.jp/frcgc/research/d5/jdannan/>

email message attachment (2007JD009373 (Editor - John Austin): Decision Letter)

From: jgr-atmospheres@agu.org
Reply-to: jgr-atmospheres@agu.org
To: jdannan@jamstec.go.jp
Subject: 2007JD009373 (Editor - John Austin): Decision Letter
Date: 11/12/07 15:40:28

plain text document attachment (2007JD009373 (Editor - John Austin): Decision Letter)

Dear Dr. Annan:

Below please find 2 reviews of your paper "Comment on 'Heat Capacity, Time Constant, and Sensitivity of Earth's Climate System,' by S. Schwartz." The reviewers have suggested revisions to your manuscript. Please take the reviewers' remarks into consideration and adequately address their questions and concerns with a revision of your manuscript. I have requested a third review and if we should receive it while you are revising your manuscript, we will notify you by e-mail. In that case, I ask that you respond to the additional comments as well.

The Reviews differed substantially in their overall assessment. Reviewer 2 is [REDACTED] with the manuscript [REDACTED]. Reviewer 1 has expressed [REDACTED].

[REDACTED]. The Reviewer indicates that the Comment should be [REDACTED].

Please submit your revised manuscript and a detailed response to each question and comment of the reviews. The revised manuscript must be returned within one month of receipt of this letter. Failure to meet this deadline may result in the revised manuscript being handled as a new submission. If you feel that you cannot address all

When you are ready to submit your revision, please use the link below.

<[http://\[REDACTED\]](http://[REDACTED])

(NOTE: The link above automatically submits your login name and password. If you wish to share this link with co-authors or colleagues, please be aware that they will have access to your entire account for this journal.)

Please note that all parts of the manuscript must be double-spaced and single-sided (including references, figure captions, and tables). Also, the references need to be on a page of their own, separated from the text of the manuscript. For further information on all editorial policies, please see our homepage at <http://www.gfdl.noaa.gov/~jaa/JGR-Atmospheres.html>

Thank you for choosing the Journal of Geophysical Research - Atmospheres.

Sincerely,

John Austin
Editor, JGR-Atmospheres

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http://www.agu.org/cgi-bin/ms_status/ms_status.cgi

*****END*****

Reviewer Comments

Reviewer #1 Evaluations:

Assessment: [REDACTED]

Ranking: [REDACTED]

Annotated Manuscript: [REDACTED]

Reviewer #1 (Comments):

[REDACTED]

Reviewer #2 Evaluations:

Assessment: [REDACTED]

Ranking: [REDACTED]

Annotated Manuscript: [REDACTED]

Reviewer #2 (Comments):

REVIEW OF FOSTER, ANNAN, SCHMIDT and MANN MANUSCRIPT

[REDACTED]

To: Gavin Schmidt <gschmidt@giss.nasa.gov>
Subject: Re: List of earth science bloggers
Date: 11/15/07 16:06:17

Thanks, Gavin. And that's a great point about over-doing the mass mailing. My intention is to spread the word about some upcoming web shorts (stand alone videos and vodcasts) that show science "in progress" and high-definition data visualizations, etc. I'll have to do more research to locate the writers who will actually find those interesting and not a bother.

Best,
Sarah

>Hi Sarah,

>

>well, the other opinion list on RC is probably a good start. Dot Earth
>(Andy Revkin), Michael Tobis, Atmoz (N. Johnson), Deltiod (Tim Lambert),
>OpenMind etc.

>

>However, I would council you not to overdo any mass mailings to these
>people - bloggers tend to be a little more independent than normal
>journalists and aren't particularly disposed to reporting on press
>releases and the like.

>

>Gavin

>

>On Thu, 2007-11-15 at 15:51, Sarah Dewitt wrote:

>> Hi Gavin,

>>

>> I'm an earth science media producer at Goddard and am starting a
>> distribution list for top earth science and climate bloggers. I'm
>> hoping you can help me kickstart the list with a few of your
>> favorites. I read Real Climate when I can but I usually don't have
>> time to follow any of the others out there, so any recommendations
>> you can make are welcome. Once I have a short list I'll put some
>> more development time into it and would be happy to share what I find.

>>

>> Thanks in advance,

>> Sarah

>>

To: Grant Foster <grant.foster@>
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>, Michael E. Mann <mann@psu.edu>
Subject: Re: more rubbish....
Date: 11/28/07 21:33:00

Grant Foster wrote:

> Life's too short to spend a career auditing other people's mistakes
> (ClimateAudit!). I do think it merits a refutation on RC.

Well I think my equation as I wrote it was a little careless in the way I was doing the differencing but the idea was sound. When I bother to define the gradients carefully it works just fine....at least for a simple AR1 model with smoothly-varying forcing and no noise at all. The attached pic shows an arbitrary forcing profile in the top plot (rise and plateau), the temperature change according to the zero-d energy balance (with a sensitivity of 3C and time scale of 15 years as per the Schwartz example). Using consecutive decadal chunks of the forcing and output my formula gives sensitivity estimates of:

3.22171	3.05286	3.02310	3.01245	3.00780
3.00558				
3.00449	3.00394	3.00366	3.00353	2.91941
2.91942				
2.91944	2.91952	2.91949	2.92011	2.92005
2.91521				
2.92353				

which is not too bad considering the approximation involved in the acceleration of the temperature.

As soon as I add just a spot of "natural variability" noise (model output in lower plot, and I think the noise is too little to be plausible) the estimates go all over the shop:

2.94261	0.785302	1.61063	1.41855	48.5832
7.40433				
1.11881	-7.97828	1.12531	1.11626	3.07257
1.68743				
-0.232121	0.345386	0.0889629	1.51644	0.761121
0.343603				
-0.552126				

I too vote for a RC post (and will embargo commenting myself in the meantime) but really want to get the schwartz thing off my desk first.

James

--

James D Annan jdannan@jamstec.go.jp Tel: +81-45-778-5618 (Fax 5707)
Frontier Research Centre for Global Change, JAMSTEC
3173-25 Showamachi, Kanazawa-ku, Yokohama City, Kanagawa, 236-0001 Japan
<http://www.jamstec.go.jp/frcgc/research/d5/jdannan/>

PS document attachment (chylek.ps)

To: James Annan <jdannan@jamstec.go.jp>, Gavin Schmidt <gschmidt@giss.nasa.gov>

Cc: Michael E. Mann <mann@psu.edu>

Subject: RE: more rubbish....

Date: 11/28/07 09:52:37

James you've done an outstanding job instituting the requested changes *and* replying to the reviewers' comments. Thanks.

Basically, I like it. [REDACTED]

Frankly I don't think we need to go further. [REDACTED]

As for the Chylek paper, it does seem a bit disheartening that there's such an assault on sound science. I've seen some of his other work (on Greenland temperatures) and he's certainly no sophisticate when it comes to statistics, but it was nothing like this latest. It makes me wonder, what the hell is up with JGR? But I try to keep perspective; the astronomical and mathematical literature certainly have their fair share of junk too. Of course, junk astronomy and mathematics don't concern the habitability of our planet.

Life's too short to spend a career auditing other people's mistakes (ClimateAudit!). I do think it merits a refutation on RC.

Grant

10. Drew Shindell <dshindell@giiss.nasa.gov>

Subject: Re: temperature trends

Date: 01/03/08 13:19:41

first draft. there is a sig difference between the models - but I need to do a better job on the stats and T4/T2 issues.

gavin

On Thu, 2008-01-03 at 12:20, Drew Shindell wrote:

> Thanks. From Miller et al (the AR4 annular modes):

>

> With stratospheric ozone

> GFDL CM2.0

> GFDL CM2.1

> GISS EH

> GISS ER

> NCAR CCSM3.0

> NCAR PCM1

> MIROC MedRes

> MPI ECHAM5

> UKMO HadGem1

> UKMO HadCM3

>

> Without strat ozone

> GISS Russell

> CCCMA CGCM3.1

> MRI CGCM2

> IAP FGOALS

>

> If those are in Santer et al data, it'd be great to look at T4 and T2
> if you have the latter. I'm not sure exactly what T_Fu is.

>

> Thanks again,

> Drew

>

> On Jan 3, 2008, at 12:05 PM, Gavin Schmidt wrote:

>

> > I did. I just used the data in the Douglass paper (so it plots the 2

> > sigma of the spread of ensemble means - which isn't really correct,

> > but

> > is good enough).

> >

> > I have more data from Santer et al 2005 which is all of the model

> > runs,

> > but only for T4, T_Fu, T2LT etc. If you have a list of all the ones

> > that

> > had ozone depletion vs not, I could have a look.

> >

> > If you are running long runs, and especially coupled models, you

> > should

> > use variable_lakes=1.

> >

> > Gavin

> >

> > On Thu, 2008-01-03 at 11:43, Drew Shindell wrote:

> >> Hi Gavin,

> >> Who made the plot on RealClimate showing that when you sample the

> >> encompassing the RA0BCORE data? What I was wondering was if the model
> >> data were readily available, would one see a difference if you looked
> >> at those with ozone trends vs those without (as in Ron's SAM
> >> results)?
> >>
> >> Another small question. What do you think about using the 'variable
> >> lake option' which Reto suggests might deal with the frozen version's
> >> tendency to create deep lakes and enormous snow depths (not sure if
> >> the latter is related to lakes or not)?
> >>
> >> Thanks,
> >> Drew
> >>
> >>
>

||PEG image attachment (ozone.jpg)

To: Drew Shindell <dshindell@giss.nasa.gov>

Subject: Re: temperature trends

Date: 01/03/08 12:05:08

I did. I just used the data in the Douglass paper (so it plots the 2 sigma of the spread of ensemble means - which isn't really correct, but is good enough).

I have more data from Santer et al 2005 which is all of the model runs, but only for T4, T_Fu, T2LT etc. If you have a list of all the ones that had ozone depletion vs not, I could have a look.

If you are running long runs, and especially coupled models, you should use `variable_lakes=1`.

Gavin

On Thu, 2008-01-03 at 11:43, Drew Shindell wrote:

> Hi Gavin,

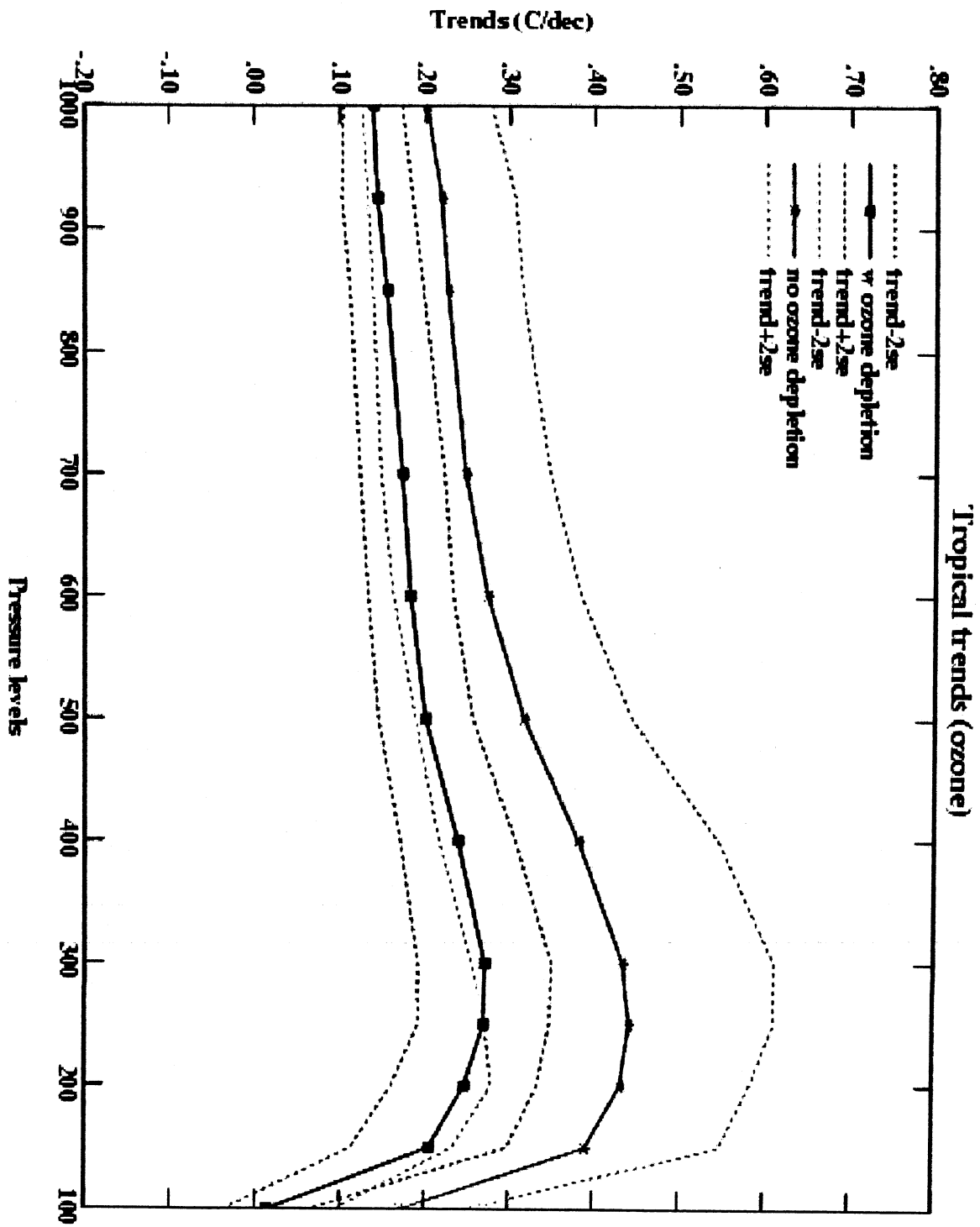
> Who made the plot on RealClimate showing that when you sample the
> AR4 models using the whole range of results it gives an envelope
> encompassing the RAOBCORE data? What I was wondering was if the model
> data were readily available, would one see a difference if you looked
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> lake option' which Reto suggests might deal with the frozen version's
> tendency to create deep lakes and enormous snow depths (not sure if
> the latter is related to lakes or not)?

>
> Thanks,

> Drew

>
>



to: Drew Shindell <dshindell@giiss.nasa.gov>

Subject: Re: temperature trends

Date: 01/03/08 13:35:33

I used the trends at specific heights, and the results as reported in Douglass et al.

the error bars are two standard errors assuming that each model group is independent, and there are 5 models that they included that aren't in your list which I didn't use.

However, you need to be careful that a) no ozone might also mean no volcanoes, so a clean attribution might not be available. b) this is for 1979-1999.

I should be able to do a better job with T4 or T2....

gavin

On Thu, 2008-01-03 at 13:27, Drew Shindell wrote:

> Wow, that's a much clearer result than I'd expected. I'm not sure
> what issues you mean as I don't know how you've gone from averaging
> kernels to height, but it looks very encouraging. If you can send me
> a ps version when you have something you're comfortable, I'll see if
> maybe we can get this included. Is this 20S to 20N b the way? And
> what time period?

>

> Thanks,

> Drew

>

> On Jan 3, 2008, at 1:19 PM, Gavin Schmidt wrote:

>

> > first draft. there is a sig difference between the models - but I need
> > to do a better job on the stats and T4/T2 issues.

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> > gavin

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> > On Thu, 2008-01-03 at 12:20, Drew Shindell wrote:

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> >> CCCMA CGCM3.1

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> >> If those are in Santer et al data, it'd be great to look at T4 and T2
> >> if you have the latter. I'm not sure exactly what T_Fu is.
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> >> Thanks again,
> >> Drew
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> >> On Jan 3, 2008, at 12:05 PM, Gavin Schmidt wrote:
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> >>> I did. I just used the data in the Douglass paper (so it plots the 2
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> >>> I have more data from Santer et al 2005 which is all of the model
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> >>>> tendency to create deep lakes and enormous snow depths (not sure if
> >>>> the latter is related to lakes or not)?
> >>>>
> >>>> Thanks,
> >>>> Drew
> >>>>
> >>>>
> >>
> >> <ozone.jpg>
>

to: Drew Shindell <dsindell@giss.nasa.gov>

Subject: Re: temperature trends

Date: 01/03/08 14:49:40

So for the T2, T4 results we have 14 runs with no ozone, 35 with:

T2:

No ozone: Mean: 0.2305 SD: 0.1304 (deg/dec)

w/ ozone: Mean: 0.1702 SD: 0.0931 (deg/dec)

=> 0.23 +/- .04 vs 0.17 +/- .02

T4:

No ozone: Mean: -0.0680 SD: 0.0964 (deg/dec)

w/ ozone: Mean: -0.2311 SD: 0.1143 (deg/dec)

=> -0.07 +/- .03 vs -0.23 +/- .02

which I think is clearly significant (you might want to do a t-test or something).

Gavin

On Thu, 2008-01-03 at 13:27, Drew Shindell wrote:

> Wow, that's a much clearer result than I'd expected. I'm not sure
> what issues you mean as I don't know how you've gone from averaging
> kernels to height, but it looks very encouraging. If you can send me
> a ps version when you have something you're comfortable, I'll see if
> maybe we can get this included. Is this 20S to 20N b the way? And
> what time period?

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> On Jan 3, 2008, at 1:19 PM, Gavin Schmidt wrote:

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> > to do a better job on the stats and T4/T2 issues.

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> >

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> >> WITHOUT STRAT OZONE
> >> GISS Russell
> >> CCCMA CGCM3.1
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> >>> but only for T4, T_Fu, T2LT etc. If you have a list of all the ones
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> >>>> Thanks,
> >>>> Drew
> >>>>
> >>>>
> >>
> >> <ozone.jpg>

To: Drew Shindell <dshindell@giss.nasa.gov>
Subject: Re: temperature trends
Date: 01/03/08 15:35:35

you should thank Ben Santer - he did all the analysis, I'm just sitting on top of it.

On Thu, 2008-01-03 at 15:25, Drew Shindell wrote:

> I think it's just a weighting issue. They average all runs from one
> group together first if I understand correctly, then calculate model
> mean. The time period is the same.

>

> Thanks for this.

>

> Drew

>

> On Jan 3, 2008, at 3:20 PM, Gavin Schmidt wrote:

>

> > is it a timescale issue? 1979-1999

> > weighting issue? what did they do about ensembles?

> >

> > Here is the data for each individual run, feel free to play around:

> >

> > Model_Run_(no_ozone) T2_Trend_1979-1999 1-sigma-trend-
> > error

> > =====

> > CCCMA3.1_20c3m_run1	0.43548E-01	1.91073E-03
> > CNRM3.0_20c3m_run1	0.27551E-01	5.17494E-03
> > GISS_AOM_A1-20C3M-run1	0.18906E-01	1.14476E-03
> > GISS_AOM_A1-20C3M-run2	0.18592E-01	9.79590E-04
> > IAP_FGOALS1.0_20c3m_run1	0.27219E-01	5.99458E-03
> > IAP_FGOALS1.0_20c3m_run2	0.24070E-01	6.25605E-03
> > IAP_FGOALS1.0_20c3m_run3	0.61048E-02	6.02062E-03
> > INMCM3.0_20c3m_run1	-0.13165E-02	3.43688E-03
> > IPSL_CM4_20c3m_run1	0.23534E-01	3.76744E-03
> > MRI2.3.2a_20c3m_run1	0.27459E-02	3.33013E-03
> > MRI2.3.2a_20c3m_run2	0.32856E-01	3.81365E-03
> > MRI2.3.2a_20c3m_run3	0.28383E-01	4.11175E-03
> > MRI2.3.2a_20c3m_run4	0.35149E-01	3.94082E-03
> > MRI2.3.2a_20c3m_run5	0.35334E-01	3.33432E-03

> >

> > Mean: 0.2305 Var: 0.0170 SD: 0.1304 Number: 14 (deg/dec)

> >

> > Model_Run_(ozone) T2_Trend_1979-1999 1-sigma-trend-error

> > =====

> > CCSM3.0_VSGSu0_b30.030a	0.21487E-01	3.17940E-03
> > CCSM3.0_VSGSu0_b30.030b	0.18276E-01	2.92237E-03
> > CCSM3.0_VSGSu0_b30.030c	0.71625E-02	3.23292E-03
> > CCSM3.0_VSGSu0_b30.030d	0.68214E-02	2.53509E-03
> > CCSM3.0_VSGSu0_b30.030e	0.24225E-01	2.54226E-03
> > CSIRO3.0_20c3m_run1	0.23913E-01	4.18092E-03
> > ECHAM5_VSGSu0_run1	0.18245E-01	4.87473E-03
> > ECHAM5_VSGSu0_run2	0.14540E-02	6.48307E-03
> > ECHAM5_VSGSu0_run3	0.15014E-01	5.02424E-03
> > GFDL2.0_CM2Q-d2-AllForc_h1	0.19950E-01	4.63987E-03
> > GFDL2.0_CM2Q-d2-AllForc_h2	0.13729E-01	4.83700E-03
> > GFDL2.0_CM2Q-d2-AllForc_h3	0.34745E-01	4.70914E-03
> > GFDL2.1_CM2.1U-d4-AllForc_h1	0.48080E-01	6.78835E-03

```

CM2.1U-d4-AllForc_n3      0.13311E-01    3.07203E-03
  H_A1-20C3M-run1         0.18433E-01    2.23950E-03
  EH_A1-20C3M-run2        0.94051E-02    2.37086E-03
  EH_A1-20C3M-run3        0.13076E-01    2.61941E-03
  S_EH_A1-20C3M-run4       0.12645E-01    2.32468E-03
  SS_EH_A1-20C3M-run5      0.12757E-01    2.08510E-03
  GISS_ER_A1-20C3M-run1    0.15641E-01    1.61538E-03
  GISS_ER_A1-20C3M-run2    0.15011E-01    1.67936E-03
  GISS_ER_A1-20C3M-run3    0.14241E-01    1.54260E-03
> GISS_ER_A1-20C3M-run4    0.22903E-01    1.76542E-03
> GISS_ER_A1-20C3M-run5    0.19678E-01    1.54528E-03
> > HADCM3_20c3m_run1      0.15629E-01    4.32915E-03
> > HADGEM1_20c3m_run2     0.28225E-01    2.17546E-03
> > MIROC3.2_T106_VSGSu0_run1 0.18099E-01    2.28582E-03
> > MIROC3.2_T42_VSGSu0_run1 0.75008E-02    2.96316E-03
> > MIROC3.2_T42_VSGSu0_run2 0.76354E-02    3.56368E-03
> > MIROC3.2_T42_VSGSu0_run3 0.92031E-02    3.81752E-03
> > PCM_VSGSu0_B06.57       0.40260E-02    2.69245E-03
> > PCM_VSGSu0_B06.59       0.13794E-01    3.04584E-03
> > PCM_VSGSu0_B06.60       0.16999E-01    2.86934E-03
> > PCM_VSGSu0_B06.61       0.18795E-01    2.54894E-03
> >
> > Mean:   0.1702 Var:   0.0087 SD:   0.0931 Number: 35 (deg/dec)
> >
> > Ozone split:
> >
> > Model_Run_no_ozone      T4_Trend_1979-1999 1-sigma-trend-error
> > =====
> > CCCMA3.1_20c3m_run1     -0.81132E-02    3.66515E-03
> > CNRM3.0_20c3m_run1      0.12655E-01    3.82667E-03
> > IAP_FGOALS1.0_20c3m_run1 0.51983E-03    3.56715E-03
> > IAP_FGOALS1.0_20c3m_run2 -0.14159E-02    3.56948E-03
> > IAP_FGOALS1.0_20c3m_run3 -0.54043E-02    3.55357E-03
> > GISS_AOM_A1-20C3M-run1   -0.31612E-02    1.97678E-03
> > GISS_AOM_A1-20C3M-run2   -0.47696E-02    1.94497E-03
> > INMCM3.0_20c3m_run1     -0.21560E-01    4.69181E-03
> > IPSL_CM4_20c3m_run1     -0.52859E-03    4.15464E-03
> > MRI2.3.2a_20c3m_run1    -0.10622E-01    4.92028E-03
> > MRI2.3.2a_20c3m_run2    -0.19169E-01    4.73010E-03
> > MRI2.3.2a_20c3m_run3    0.13178E-02    4.77688E-03
> > MRI2.3.2a_20c3m_run4    -0.19450E-01    4.79796E-03
> > MRI2.3.2a_20c3m_run5    -0.15490E-01    4.92463E-03
> >
> > Mean:  -0.0680 Var:   0.0093 SD:   0.0964 Number: 14 (deg/dec)
> >
> > Model_Run_ozone          T4_Trend_1979-1999 1-sigma-trend-error
> > =====
> > CCSM3.0_VSGSu0_b30.030a  -0.45407E-02    9.53921E-03
> > CCSM3.0_VSGSu0_b30.030b  -0.15619E-01    9.40897E-03
> > CCSM3.0_VSGSu0_b30.030c  -0.43351E-02    9.41515E-03
> > CCSM3.0_VSGSu0_b30.030d  -0.12482E-01    9.19572E-03
> > CCSM3.0_VSGSu0_b30.030e  -0.10506E-01    1.06773E-02
> > CSIRO3.0_20c3m_run1      -0.29691E-01    2.26140E-03
> > ECHAM5_VSGSu0_run1       -0.27049E-01    4.09139E-03
> > ECHAM5_VSGSu0_run2       -0.26068E-02    4.44909E-03
> > ECHAM5_VSGSu0_run3       -0.19891E-01    3.95419E-03
> > GFDL2.0_CM2Q-d2-AllForc_h1 -0.29284E-01    4.75706E-03
> > GFDL2.0_CM2Q-d2-AllForc_h2 -0.22027E-01    4.32483E-03

```

```

> > GFDL2.1_CM2.1U-d4-AllForc_h1 -0.29940E-01 5.05005E-03
> > GFDL2.1_CM2.1U-d4-AllForc_h2 -0.37098E-01 5.81082E-03
> > GFDL2.1_CM2.1U-d4-AllForc_h3 -0.28998E-01 4.53998E-03
> > GISS_EH_A1-20C3M-run1 -0.24815E-01 6.07890E-03
> > GISS_EH_A1-20C3M-run2 -0.11756E-01 6.20856E-03
> > GISS_EH_A1-20C3M-run3 -0.22071E-01 6.13668E-03
> > GISS_EH_A1-20C3M-run4 -0.18816E-01 5.88935E-03
> > GISS_EH_A1-20C3M-run5 -0.17092E-01 6.15247E-03
> > GISS_ER_A1-20C3M-run1 -0.20049E-01 5.93597E-03
> > GISS_ER_A1-20C3M-run2 -0.21951E-01 5.17858E-03
> > GISS_ER_A1-20C3M-run3 -0.19722E-01 4.89764E-03
> > GISS_ER_A1-20C3M-run4 -0.17347E-01 5.51781E-03
> > GISS_ER_A1-20C3M-run5 -0.20716E-01 5.98200E-03
> > HADCM3_20c3m_run1 -0.34749E-01 3.34171E-03
> > HADGEM1_20c3m_run2 -0.23379E-01 8.83644E-03
> > MIROC3.2_T106_VSGSu0_run1 -0.47736E-01 7.44245E-03
> > MIROC3.2_T42_VSGSu0_run1 -0.48720E-01 6.63425E-03
> > MIROC3.2_T42_VSGSu0_run2 -0.38497E-01 6.82694E-03
> > MIROC3.2_T42_VSGSu0_run3 -0.42979E-01 6.60491E-03
> > PCM_VSGSu0_B06.57 -0.87352E-02 8.18523E-03
> > PCM_VSGSu0_B06.59 -0.22275E-01 8.80004E-03
> > PCM_VSGSu0_B06.60 -0.15899E-01 8.75161E-03
> > PCM_VSGSu0_B06.61 -0.26610E-01 9.11513E-03
> >
> > Mean: -0.2311 Var: 0.0131 SD: 0.1143 Number: 35 (deg/dec)
> >
> >
> > On Thu, 2008-01-03 at 15:14, Drew Shindell wrote:
> >> Turns out CCSP 1.1 includes results for T4 for the subset of models
> >> with O3 depletion. This causes their trend to go to -0.27, a bit more
> >> than the -0.23 you give but close. They don't look at T2 though, so
> >> that part's interesting.
> >> Drew
> >>
> >> On Jan 3, 2008, at 2:49 PM, Gavin Schmidt wrote:
> >>
> >>> So for the T2, T4 results we have 14 runs with no ozone, 35 with:
> >>>
> >>> T2:
> >>>
> >>> No ozone: Mean: 0.2305 SD: 0.1304 (deg/dec)
> >>> w/ ozone: Mean: 0.1702 SD: 0.0931 (deg/dec)
> >>>
> >>> => 0.23 +/- .04 vs 0.17 +/- .02
> >>>
> >>> T4:
> >>>
> >>> No ozone: Mean: -0.0680 SD: 0.0964 (deg/dec)
> >>> w/ ozone: Mean: -0.2311 SD: 0.1143 (deg/dec)
> >>>
> >>> => -0.07 +/- .03 vs -0.23 +/- .02
> >>>
> >>> which I think is clearly significant (you might want to do a t-
> >>> test or
> >>> something).
> >>>
> >>> Gavin

```

```

> >>>
> >>> On Thu, 2008-01-03 at 13:27, Drew Shindell wrote:
> >>>> Wow, that's a much clearer result than I'd expected. I'm not sure
> >>>> what issues you mean as I don't know how you've gone from averaging
> >>>> kernels to height, but it looks very encouraging. If you can
> >>>> send me
> >>>> a ps version when you have something you're comfortable, I'll
> >>>> see if
> >>>> maybe we can get this included. Is this 20S to 20N b the way? And
> >>>> what time period?
> >>>>
> >>>> Thanks,
> >>>> Drew
> >>>>
> >>>> On Jan 3, 2008, at 1:19 PM, Gavin Schmidt wrote:
> >>>>
> >>>>> first draft. there is a sig difference between the models - but I
> >>>>> need
> >>>>> to do a better job on the stats and T4/T2 issues.
> >>>>>
> >>>>> gavin
> >>>>>
> >>>>> On Thu, 2008-01-03 at 12:20, Drew Shindell wrote:
> >>>>>> Thanks. From Miller et al (the AR4 annular modes):
> >>>>>>
> >>>>>> With stratospheric ozone
> >>>>>> GFDL CM2.0
> >>>>>> GFDL CM2.1
> >>>>>> GISS EH
> >>>>>> GISS ER
> >>>>>> NCAR CCSM3.0
> >>>>>> NCAR PCM1
> >>>>>> MIROC MedRes
> >>>>>> MPI ECHAM5
> >>>>>> UKMO HadGem1
> >>>>>> UKMO HadCM3
> >>>>>>
> >>>>>> Without strat ozone
> >>>>>> GISS Russell
> >>>>>> CCCMA CGCM3.1
> >>>>>> MRI CGCM2
> >>>>>> IAP FGOALS
> >>>>>>
> >>>>>> If those are in Santer et al data, it'd be great to look at T4
> >>>>>> and T2
> >>>>>> if you have the latter. I'm not sure exactly what T_Fu is.
> >>>>>>
> >>>>>> Thanks again,
> >>>>>> Drew
> >>>>>>
> >>>>>> On Jan 3, 2008, at 12:05 PM, Gavin Schmidt wrote:
> >>>>>>
> >>>>>>> I did. I just used the data in the Douglass paper (so it plots
> >>>>>>> the 2
> >>>>>>> sigma of the spread of ensemble means - which isn't really
> >>>>>>> correct,
> >>>>>>> but
> >>>>>>> is good enough).

```

> >>>>>> I have more data from Sander. It is 2000-2005. It is all of the
> >>>>>> model
> >>>>>> runs,
> >>>>>> but only for T4, T_Fu, T2LT etc. If you have a list of all the
> >>>>>> ones
> >>>>>> that
> >>>>>> had ozone depletion vs not, I could have a look.
> >>>>>>
> >>>>>> If you are running long runs, and especially coupled models, you
> >>>>>> should
> >>>>>> use variable_lakes=1.
> >>>>>>
> >>>>>> Gavin
> >>>>>>
> >>>>>> On Thu, 2008-01-03 at 11:43, Drew Shindell wrote:
> >>>>>> Hi Gavin,
> >>>>>> Who made the plot on RealClimate showing that when you
> >>>>>> sample
> >>>>>> the
> >>>>>> AR4 models using the whole range of results it gives an
> >>>>>> envelope
> >>>>>> encompassing the RA0BCORE data? What I was wondering was if the
> >>>>>> model
> >>>>>> data were readily available, would one see a difference if you
> >>>>>> looked
> >>>>>> at those with ozone trends vs those without (as in Ron's SAM
> >>>>>> results)?
> >>>>>>
> >>>>>> Another small question. What do you think about using the
> >>>>>> 'variable
> >>>>>> lake option' which Reto suggests might deal with the frozen
> >>>>>> version's
> >>>>>> tendency to create deep lakes and enormous snow depths (not
> >>>>>> sure if
> >>>>>> the latter is related to lakes or not)?
> >>>>>>
> >>>>>> Thanks,
> >>>>>> Drew
> >>>>>>
> >>>>>>
> >>>>>>
> >>>>>> <ozone.jpg>
> >>>>>>
> >>>>>>
>

to: akocn@giss.nasa.gov

Subject: [Fwd: request for review]

Date: 02/01/08 16:04:16

Dorothy, do you have time to look at this?

gavin

-----Forwarded Message-----

> From: Anja Kollmuss <anja.kollmuss@sei-us.org>

> To: gschmidt@giss.nasa.gov

> Subject: request for review

> Date: 01 Feb 2008 07:33:22 -0500

>

> Dear Gavin Schmidt,

>

>

>

> I found your contact info on the realclimate website. I am a researcher
> at The Stockholm Environment Institute. My field of research is in
> cap-and-trade carbon policies and carbon offsets.

>

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> I am currently writing a paper that lies partly outside my expertise and
> I therefore would like to request your help.

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> providers and their emissions calculators. In response to this paper I
> have gotten numerous requests to comment on and explain how air travel
> GHG emissions should best be calculated to determine the number of
> offsets an individual would have to purchase to 'offset' their climate
> impact.

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> that explains to non-scientists (i.e. policy makers and offset
> providers) the parameter that need to be considered when calculating GHG
> emissions from air travel for offsetting. The first part of the paper
> explains the issue of non-CO2 emissions, the second part deals with
> air-travel related factors such as plane type, occupancy, seat class,
> etc.

>

>

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> I would like to make sure that all the information presented is correct.

> Since I am neither an atmospheric chemist nor a modeler, I need the

> first part to be reviewed by an expert.

>

>

>

> Would you be able to review the first two main chapters of the paper? If

>
>
>
> If yes, I'd be very grateful and would like to request that you use
> track changes in MS Word and send the comments back by February 10.

>
>
>
> Thank you very much!

>
>
>
> Sincerely yours,

>
>
>
> -Anja Kollmuss

>
>
>
> Anja Kollmuss

>
>
>
> Associate Scientist

>
>
>
> Stockholm Environment Institute - US

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>
>
> 11 Curtis Avenue

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>
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>
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Word document attachment (Air_Travel_Emissions_Paper 5draft.doc)
--

to: Drew Snindell <dsnindell@giss.nasa.gov>

Subject: [Fwd: request for review]

Date: 02/01/08 12:19:37

Drew, do you have time for this?

Gavin

-----Forwarded Message-----

> From: Anja Kollmuss <anja.kollmuss@sei-us.org>

> To: gschmidt@giss.nasa.gov

> Subject: request for review

> Date: 01 Feb 2008 07:33:22 -0500

>

> Dear Gavin Schmidt,

>

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> air-travel related factors such as plane type, occupancy, seat class,
> etc.

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> first part to be reviewed by an expert.

>

>

>

> Would you be able to review the first two main chapters of the paper? If


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> If yes, I'd be very grateful and would like to request that you use
> track changes in MS Word and send the comments back by February 10.
```

> Sincerely yours,

> Anja Kollmuss

Word document attachment (Air_Travel_Emissions_Paper 5draft.doc)

To: David Rind <drind@giss.nasa.gov>
Subject: [Fwd: request for review]
Date: 02/01/08 12:31:18

David, do you have time for this?

Gavin

-----Forwarded Message-----

> From: Anja Kollmuss <anja.kollmuss@sei-us.org>
> To: gschmidt@giss.nasa.gov
> Subject: request for review
> Date: 01 Feb 2008 07:33:22 -0500

> Dear Gavin Schmidt,

> I found your contact info on the realclimate website. I am a researcher
> at The Stockholm Environment Institute. My field of research is in
> cap-and-trade carbon policies and carbon offsets.

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> I therefore would like to request your help.

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> GHG emissions should best be calculated to determine the number of
> offsets an individual would have to purchase to 'offset' their climate
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> emissions from air travel for offsetting. The first part of the paper
> explains the issue of non-CO2 emissions, the second part deals with
> air-travel related factors such as plane type, occupancy, seat class,
> etc.

> I would like to make sure that all the information presented is correct.

> ~~Since I am neither an atmospheric chemist nor a modeler, I need the~~
> first part to be reviewed by an expert.

> not, do you have someone you could recommend who might be able to do so?

> If yes, I'd be very grateful and would like to request that you use
> track changes in MS Word and send the comments back by February 10.

> Thank you very much!

> Sincerely yours,

> -Anja Kollmuss

> Anja Kollmuss

> Associate Scientist

> Stockholm Environment Institute - US

> 11 Curtis Avenue

> Somerville, MA 02144-1224, USA

> Tel: +1 (617) 627-3786 8#

> Fax: +1 (617) 449-9603

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Word document attachment (Air_Travel_Emissions_Paper 5draft.doc)

To: David Rind <drind@giss.nasa.gov>

Subject: Re: [Fwd: request for review]

Date: 02/01/08 16:04:53

thanks anyway

On Fri, 2008-02-01 at 12:41, David Rind wrote:

> Gavin, not really. I've sort of gotten Dorothy
> Koch involved in the NASA 'aviation effects on
> climate' program - perhaps it would be good for
> her to read and comment on this.

>

> David

>

> >David, do you have time for this?

> >

> >Gavin

> >

> >

> >-----Forwarded Message-----

> >

> >> From: Anja Kollmuss <anja.kollmuss@sei-us.org>

> >> To: gschmidt@giss.nasa.gov

> >> Subject: request for review

> >> Date: 01 Feb 2008 07:33:22 -0500

> >>

> >> Dear Gavin Schmidt,

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> >> I found your contact info on the realclimate website. I am a researcher
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> >> very technical papers or very vague (and often faulty) guidelines
> >> available. To address this information gap, I set out to write a paper
> >> that explains to non-scientists (i.e. policy makers and offset

> >> providers) the parameter that need to be considered when calculating GHG

> >> emissions from air travel for offsetting. The first part of the paper

> >> explains the issue of non-CO2 emissions, the second part deals with

> >> air-travel related factors such as plane type, occupancy, seat class,

> >
> >
> >
> > I would like to make sure that all the information presented is correct.
> > Since I am neither an atmospheric chemist nor a modeler, I need the
> > first part to be reviewed by an expert.

> >
> >
> >
> > Would you be able to review the first two main chapters of the paper? If
> > not, do you have someone you could recommend who might be able to do so?

> >
> >
> > If yes, I'd be very grateful and would like to request that you use
> > track changes in MS Word and send the comments back by February 10.

> >
> >
> >
> > Thank you very much!

> >
> >
> >
> > Sincerely yours,

> >
> >
> >
> > -Anja Kollmuss

> >
> >
> >
> >
> > Anja Kollmuss
> > Associate Scientist
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> >
> >Content-Type: application/msword; name="Air_Travel_Emissions_Paper 5draft.doc"
> >Content-Description: Air_Travel_Emissions_Paper 5draft.doc
> >Content-Disposition: attachment;
> >filename="Air_Travel_Emissions_Paper 5draft.doc"

> >Attachment converted:
> >Toltec:Air_Travel_Emissio#1BF48CB7.doc
> >(WDBN/«IC») (1BF48CB7)
>

To: Gavin Schmidt <gschmidt@giss.nasa.gov>

Subject: Re: [Fwd: request for review]

Date: 02/04/08 16:54:59

Hi Gavin,

I would be willing to give this person some comments but not within this week as they request.

Since it fits well with the hefty white papers I'm reading now I would be much better able to give useful comments after I read those. That would be within the next 3 weeks.

Dorothy

Quoting Gavin Schmidt <gschmidt@giss.nasa.gov>:

> Dorothy, do you have time to look at this?

>

> gavin

>

> -----Forwarded Message-----

>

>> From: Anja Kollmuss <anja.kollmuss@sei-us.org>

>> To: gschmidt@giss.nasa.gov

>> Subject: request for review

>> Date: 01 Feb 2008 07:33:22 -0500

>>

>> Dear Gavin Schmidt,

>>

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>> I found your contact info on the realclimate website. I am a researcher at The Stockholm Environment Institute. My field of research is in cap-and-trade carbon policies and carbon offsets.

>>

>>

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>>

>>

>>

>> Last year I wrote a paper that compared 13 different carbon offset providers and their emissions calculators. In response to this paper I have gotten numerous requests to comment on and explain how air travel GHG emissions should best be calculated to determine the number of offsets an individual would have to purchase to 'offset' their climate impact.

>>

>>

>>

>> In my search for a good explanation I had to realize that there are only very technical papers or very vague (and often faulty) guidelines

>> available. To address this information gap, I set out to write a paper

>> that explains to non-scientists (i.e. policy makers and offset

>> providers) the parameter that need to be considered when calculating GHG

>> emissions from air travel for offsetting. The first part of the paper

Dorothy Koon
Research Scientist
Columbia University and
Goddard Institute for Space Studies
2880 Broadway, New York NY 10025

To: Dorothy Koch <dkoch@giss.nasa.gov>

Subject: Re: [Fwd: request for review]

Date: 02/08/08 13:12:50

thanks. Letting him know directly is probably best.

Gavin

On Mon, 2008-02-04 at 16:54, Dorothy Koch wrote:

> Hi Gavin,

> I would be willing to give this person some comments

> but not within this week as they request.

> Since it fits well with the hefty white papers I'm

> reading now I would be much better able to give

> useful comments after I read those. That would be

> within the next 3 weeks.

> Dorothy

>

>

> Quoting Gavin Schmidt <gschmidt@giss.nasa.gov>:

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> >

> > gavin

> >

> > -----Forwarded Message-----

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> >> From: Anja Kollmuss <anja.kollmuss@sei-us.org>

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> >> Date: 01 Feb 2008 07:33:22 -0500

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> > If yes, I'd be very grateful and would like to request that you use

> > track changes in MS Word and send the comments back by February 10.

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> >

> >

> > Thank you very much!

> >

> >

> >

> > Sincerely yours,

> >

> >

> >

> > -Anja Kollmuss

> >

> >

> >

> >

> >

> > Anja Kollmuss

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>
>
> Dorothy Koch
> Research Scientist
> Columbia University and
> Goddard Institute for Space Studies
> 2880 Broadway, New York NY 10025
>
>

to: Daven Henze <uknz11@...>

Subject: Re: El Seminar 2/19, Gavin Schmidt: "Communicating Climate Science: Tiptoeing through the minefield"

Date: 02/18/08 15:07:15

thanks for the reminder. I'd almost forgotten!

see you tomorrow.

Gavin

On Mon, 2008-02-18 at 14:03, Daven Henze wrote:

> Tuesday, Feb 19th

> Hogan Conference room

> 12:00 - 2:00

> Food -- provided or bring your own? Still not sure about this; look

> for another email tomorrow.

>

>

> Gavin Schmidt is a climate scientist at NASA GISS. His expertise in

> science communication is evident from the success of a climate blog he

> maintains with several colleagues, www.realclimate.org. This site has

> been noted by Science, Scientific American and Nature as

> an exemplary resource.

>

>

>

> =====

>

> Communicating Climate Science: Tiptoeing through the minefield"

>

> Abstract:

>

> Climate change is example of a science that, given the large perceived

> impacts, has become highly politicized. In such an environment,

> science

> is often used within the political context as a proxy for political

> positions. This 'science' is often uncontextualised, over-interpreted

> and frequently has nothing to do with the political debate at hand.

> Public statements by scientists--whether in media interviews, press

> releases or in briefings very often become fodder for political

> discussion in ways that are frequently contrary to the positions held

> by

> the scientists themselves. This 'scientization' of the political

> discourse places scientists in a very delicate position.

>

> How far do scientists' responsibilities go in ensuring that relevant

> science is appropriately transmitted and understood by the public and

> policy makers? Even if scientists are not interested in the political

> ramifications of their work, do they still have a responsibility to

> try

> and ensure that it is not misused? What recourses are available to

> extract work from the fake 'scientized' political debate? Do all

> scientists have this responsibility, or can the field rely on a few

> public spokespeople? To what extent are 'public' scientists

> responsible

> for explaining/defending the field as a whole rather than just their

> own

> I will try to make the case that simple publication in the technical
> literature is clearly not sufficient, but that attempts at
> popularization of the science (whether through traditional media or
> blogs) is fraught with problems of its own. Examples of unfortunate
> public statements and subsequently appalling media coverage are
> legion.

> On Jan 29, 2008, at 3:08 PM 01/29/08, Kenny Shirley wrote:

> > Hi all,

> > Pending confirmation, Daven has a speaker lined up for 2/19: Gavin
> > Schmidt, a climate scientist at NASA GISS who, among other things,
> > authors a popular blog you might want to check out
> > (www.realclimate.org). Daven can supply more speaker info, links,
> > etc. as the date approaches...

> > This is great! Looking forward to filling up the other slots.

> > kenny

> > Spring 2008 Fellows Seminar Dates:

> > > 2/5

> > > 2/19 : Gavin Schmidt (Climate Scientist, NASA GISS)

> > > 3/4

> > > 3/18

> > > 4/1

> > > 4/15

> > > **Monday (4/21)** symposium

> > > 4/29

> > > 5/13

Reply-to: santer1@llnl.gov

To: Thorne, Peter <peter.thorne@metoffice.gov.uk>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, 'Susan Solomon' <ssolomon@al.noaa.gov>, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, 'Philip D. Jones' <p.jones@uea.ac.uk>, Karl Taylor <taylor13@llnl.gov>, Steve Klein <klein21@mail.llnl.gov>, carl mears <mears@remss.com>, Doug Nychka <nychka@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>

Subject: [Fwd: Re: JOC-08-0098 - International Journal of Climatology]

Date: 04/25/08 16:19:18

Dear folks,

On April 11th, I received an email from Prof. Glenn McGregor at IJoC. I am now forwarding that email, together with my response to Prof. McGregor.

Prof. McGregor's email asks for my opinion of an "Addendum" to the original DCPS07 IJoC paper. The addendum is authored by Douglass, Christy, Pearson, and Singer. As you can see from my reply to Prof. McGregor, I do not think that the Addendum is worthy of publication. Since one part of the Addendum deals with issues related to the RAOBCORE data used by DCPS07 (and by us), Leo responded to Prof. McGregor on this point. I will forward Leo's response in a separate email.

The Addendum does not reference our IJoC paper. As far as I can tell, the Addendum represents a response to discussions of the original IJoC paper on RealClimate.org. Curiously, Douglass et al. do not give a specific source for the criticism of their original paper. This is rather bizarre. Crucially, the Addendum does not recognize or admit ANY ERRORS in the original DCPS07 paper.

I have not yet heard whether IJoC intends to publish the Addendum. I'll update you as soon as I have any further information from Prof. McGregor.

With best regards,

Ben

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

PDF document attachment

(addendum-A-comparison-of-tropical-temperature-trends-with-model-JOC1651-s1-ln377204795844769-1939656818Hwf-8

email message attachment (Re: JOC-08-0098 - International Journal of Climatology)

From: Ben Santer <santer1@llnl.gov>

Reply-to: santer1@llnl.gov

To: g.mcgregor@auckland.ac.nz

<peter.thorne@metoffice.gov.uk>

Subject: Re: JOC-08-0098 - International Journal of Climatology

Date: 04/11/08 14:14:37

plain text document attachment (Re: JOC-08-0098 - International Journal of Climatology)

to: Gavin Schmidt <gsmith@giiss.nasa.gov>

Subject: Re: MM07 and dLM06

Date: 05/16/08 03:13:34

Hi,

I read your manuscript - nice demonstration!

There are a few minor points which maybe do not come out as clearly as it perhaps could. On the bottom of p. 7 and top of p. 8 you mention 'method 1' (the area-weighted mean of trends) and 'method 2' (trend of the area weighted mean temperatures). Are the conclusions sensitive to this choice?

p. 9, first to lines in 2.1: is it possible to show that emissions in 1990 can represent industrial activity since 1940? (are there time series for emissions going back to then? References?). As far as the test is concerned, this does not really matter, because you can show that the correlations fluctuate strongly due to internal chaotic variations and hence the spread. I presume that you have used different 23-year periods for the temperature only?

p. 13, l. 4: should 'than' be a 'to' at the end of the line?

p. 14, l. 2: the symbol '3' should probably not be there.

You can also mention that MM07 obtained largest correction factors for remote Arctic places like Svalbard, the South Orkney Island, at a site somewhere 'near' the North-West passage, and in the mountain region near Ashgabat(!). Attached is a Google Earth view of the area.

(<http://www.realclimate.org/images/MM2007.jpg>)

Rasmus

Gavin Schmidt wrote:

> Hi Rasmus, If you recall, I discussed writing a paper on the silly M&M07
> paper along with a critique of de Laat and Maurelis 06. Attached is a
> draft of that analysis which concludes (unsurprisingly) that the
> significance is very low.

>

> Since you dealt with this previously, I'd appreciate any comments you
> would have on this draft. The main issue I see going ahead is finding a
> better way to assess the spatial correlations and thus reducing the
> degrees of freedom in their analyses. Any pointers would be welcome!

>

> Thanks

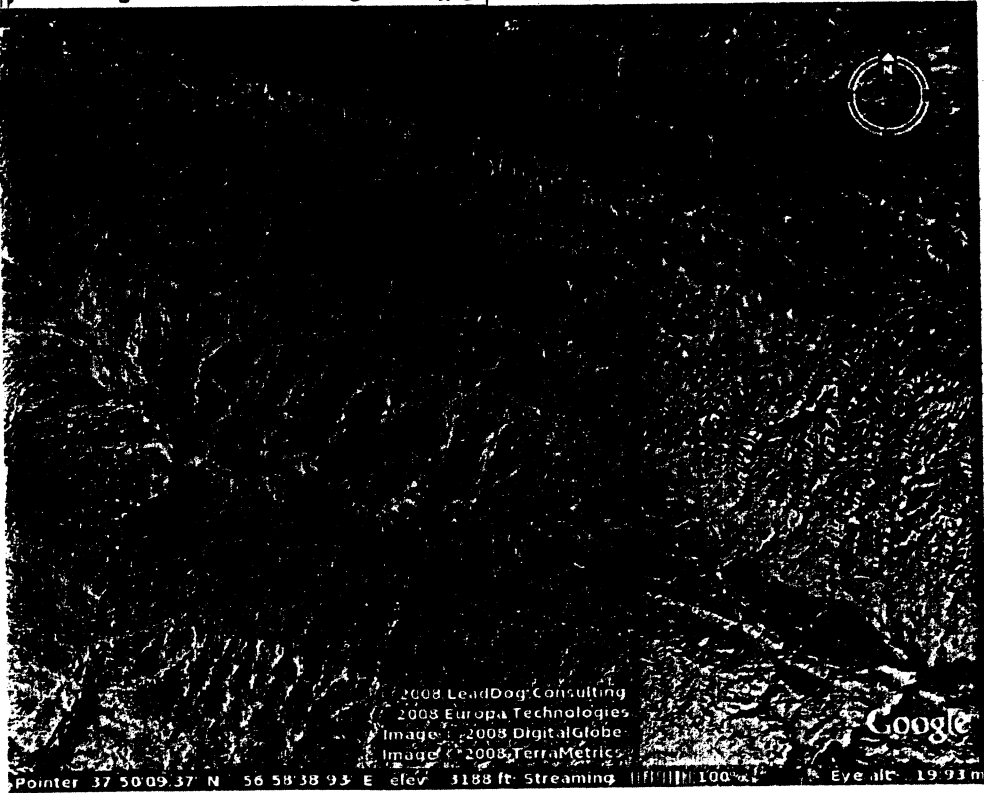
>

> Gavin

[[JPEG image attachment (ashgabat.jpg)]]



PEG image attachment (ashgabat2.jpg)



To: Sarah Galloway <galloway@amnh.org>
Subject: Re: AMNH Video for Climate Change Exhibit
Date: 06/16/08 17:08:35

july 25 is ok.

Gavin

On Mon, 2008-06-16 at 17:00, Sarah Galloway wrote:

> Hi Gavin,
>
> I just tried this number: 212/678-5627 and left a voice mail.
>
> In general, we're aiming to tape our interviews later in July. If I
> had to pick a top date right now it would be July 25th, but we will
> have 2-3 videotaping days in order to fit in each of our interviews.
>
> Your interview would require a half day commitment. This includes set
> up as well as the interview itself which should last no more than 1.5
> hours.
>
> Would you be available July 25th or another date in July?
>
> I'd like to hear from you in the next few days so I may plan our video
> taping days.
>
> Call me at 212/496-3461 or e-mail back.
>
> Best regards,
>
> Sarah
>
>
> Sarah Galloway, Senior Media Producer
> Exhibition Department
> The American Museum of Natural History
> Central Park West @ 79th Street
> New York, NY 10024-5192
> Tel: (212) 496-3461
> Fax: (212) 769-5426
> E-mail: galloway@amnh.org

> On 11 Jun, 2008, at 12:53 PM, Gavin Schmidt wrote:

> >
> >
> > Sounds great. I'm glad to hear this is proceeding and I'd love to
> > help out. I'm out of town until next week, but call me then so that
> > we can discuss details.

> >
> > Gavin

> > *-----*

> > | Gavin Schmidt NASA/Goddard Institute for Space Studies

> > |
> > | 2880 Broadway

> > |
> > | Tel: (212) 678 5627 New York, NY 10025

> > |

> > |
> > | gschmidt@giss.nasa.gov http://www.giss.nasa.gov/~gavin
> > |
> > *-----*
> >
> > On Wed, 11 Jun 2008, Sarah Galloway wrote:
> >
> > > Dear Dr. Gavin Schmidt,
> > >
> > > I am contacting you about participating in a video that will be
> > > displayed in the American Museum of Natural History's upcoming
> > > exhibit on Climate Change, opening in October 2008. For this
> > > exhibit we are creating a video program that will focus visitor
> > > attention on group responses to climate change – on a national and
> > > global scale. The program's working title is "Rethinking Our
> > > Energy Future" and it will feature interviews with climate
> > > scientists and other experts who will speak about clean energy
> > > technologies, government policy, economics, and the risks involved
> > > in facing climate change.
> > >
> > > As the video outlines the challenges that lie ahead, it also
> > > reminds us that we have successfully faced global challenges
> > > before. Human activity has been a contributing factor to climate
> > > change, and human ingenuity and perseverance will be the guiding
> > > force in creating a more sustainable future.
> > >
> > > As your work in climate modeling and your outreach work with
> > > RealClimate.org represents a positive response to climate change,
> > > we would very much like to include your voice in this program, to
> > > help bring this message to our visitors.
> > >
> > > At this time I'd like to see if you have an interest in
> > > participating and are available for an on-camera interview for
> > > "Rethinking Our Energy Future". Video production will take place
> > > in July and early August of this year.
> > >
> > > I'd like to have an opportunity to discuss the project with you in
> > > more detail. Please don't hesitate to call or e-mail with any
> > > questions or concerns you may have. I'll follow up with a phone
> > > call shortly.
> > >
> > > Best regards,
> > >
> > > Sarah
> > >
> > >
> > >
> > > Sarah Galloway, Senior Media Producer
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> > > Fax: (212) 769-5426
> > > E-mail: galloway@amnh.org
> > > Web site: AMNH.org
> > >
>

To: Clune, Thomas L. (GSFC-610.3) <thomas.l.clune@nasa.gov>

Cc: Putman, William M. (GSFC-610.3) <william.m.putman@nasa.gov>, gschmidt@giss.nasa.gov,
Maxwell Kelley <mk98@columbia.edu>

Subject: Re: FV core

Date: 06/19/08 14:11:00

Hi Tom, Hi Bill,

It was nice meeting you over the phone yesterday.

I will need to talk with Bill about regridding, mostly on the following topics:

* How do I get the regridding code that was implemented with (or at) GFDL?

* What does the code do? My understanding is that it provides us with interpolation and integration routines ensuring that quantities which are globally conserved on one grid are still conserved on the other.

Thanks,

Denis

On Thu, Jun 19, 2008 at 1:46 PM, Clune, Thomas L. (GSFC-610.3)
<thomas.l.clune@nasa.gov> wrote:

> All,
> Apologies for the delay in creating the Blog on MG. Turns out both admins
> for MG are out today ... We could start it as a "disussion" but I think
> we'll ultimately want the extra flexibility of a blog for this purpose. If
> you'd rather not wait another day, just let me know.

> Cheers,
> - Tom

> On Jun 17, 2008, at 2:37 PM, Denis Gueyffier wrote:

> Bill, Tom,

> Is it possible for you to call me (phone: 212-678-5572) from your phone?
> Max Kelley will also join us for the meeting.

> Best,

> Denis Gueyffier
> NASA GISS & Columbia CCSR
> 2880 Broadway
> New York, NY 10025
> E-mail: dgueyffier@giss.nasa.gov
> Phone: 212-678-5572
> Office: 678

> On Mon, Jun 16, 2008 at 3:25 PM, Denis Gueyffier

> <dgueyffier@giss.nasa.gov> wrote:

>> Wednesday 1:00 is fine with me.

>> Best,

>> Denis

>> On Mon, Jun 16, 2008 at 2:29 PM, William Putman

>>>
>>> 1:00 is fine with me.
>>> Denis, can you make 1:00 wednesday?
>>>
>>> William.M.Putman@nasa.gov
>>>
>>> NASA - Goddard Space Flight Center
>>>
>>> Code 610.3, Software Integration and Visualization Office
>>>
>>> Greenbelt, MD 20771
>>>
>>> Cell Phone: 240-778-5697
>>>
>>> Fax: 240-266-1105
>>>
>>> On Jun 16, 2008, at 2:27 PM, Clune, Thomas L. (GSFC-610.3) wrote:
>>>
>>> 1:00 on Wednesday is my only remaining slot.
>>> - Tom
>>> On Jun 16, 2008, at 2:00 PM, Putman, William M. (GSFC-610.3) wrote:
>>>
>>> Denis, Tom, perhaps we can schedule to talk on wednesday, either late
>>> morning or after lunch.
>>> -Bill
>>> William.M.Putman@nasa.gov
>>> NASA - Goddard Space Flight Center
>>> Code 610.3, Software Integration and Visualization Office
>>> Greenbelt, MD 20771
>>> Cell Phone: 240-778-5697
>>> Fax: 240-266-1105
>>>
>>> On Jun 9, 2008, at 5:21 PM, Denis Gueyffier wrote:
>>>
>>> Hi Tom, Hi Bill,
>>> Thanks for your email. I'd be glad to speak with you any time that is
>>> convenient for you next Monday.
>>> In the meantime, I was planning to read a few papers by Bill and S. J.
>>> Lin, and also earlier papers by Colella & Woodward.
>>> Please tell me if you have other pointers about things that you think I
>>> should look at (papers, presentations, codes...).>>> Thanks,
>>> Denis Gueyffier
>>>
>>> On Mon, Jun 9, 2008 at 5:02 PM, Clune, Thomas L. (GSFC-610.3)
>>> <Thomas.L.Clune@nasa.gov> wrote:
>>>>
>>>> Hi Dennis,
>>>> Glad to e-meet you. Please don't take our relative silence
>>>> personally. Bill is on travel to the US Virgin Islands this week, and was
>>>> in Boulder last week. For myself I'm just about to leave for a few days of
>>>> vacation and was inundated with a few deliverables for NASA HQ. Let's plan
>>>> to touch base next Monday, which is currently completely open on my
>>>> calendar.

>>>> Cheers,
>>>> - Tom
>>>>
>>>>

>
> Thomas Clune, Ph.D. 301-286-4635 (W)
> Advanced Software Technology Group 240-266-0400 (F)
> Software Integration and Visualization Office <Thomas.L.Clune@nasa.gov>
> NASA GSFC (610.3) <<http://sivo.gsfc.nasa.gov>>
>
>
>
>

To: Denis Gueymer <dgueymer@giss.nasa.gov>

Cc: Putman, William M. (GSFC-610.3) <william.m.putman@nasa.gov>, gschmidt@giss.nasa.gov,
Maxwell Kelley <mk98@columbia.edu>

Subject: Re: FV core

Date: 06/19/08 13:46:04

All,

Apologies for the delay in creating the Blog on MG. Turns out both admins for MG are out today ... We could start it as a "disussion" but I think we'll ultimately want the extra flexibility of a blog for this purpose. If you'd rather not wait another day, just let me know.

Cheers,

- Tom

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2880 Broadway
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E-mail: dgueyffier@giss.nasa.gov
Phone: 212-678-5572
Office: 678

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>> On Jun 16, 2008, at 2:27 PM, Clune, Thomas L. (GSFC-610.3) wrote:

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>> On Jun 9, 2008, at 5:21 PM, Denis Gueyffier wrote:

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>> In the meantime, I was planning to read a few papers by Bill and S. J. Lin, and also earlier papers by Colella & Woodward.

>> Please tell me if you have other pointers about things that you think I should look at (papers, presentations, codes...).

>> Thanks,

>> Denis Gueyffier

>>

>> On Mon, Jun 9, 2008 at 5:02 PM, Clune, Thomas L. (GSFC-610.3) <Thomas.L.Clune@nasa.gov> wrote:

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>>> Hi Dennis,

>>> Glad to e-meet you. Please don't take our relative silence personally. Bill is on travel to the US Virgin Islands this week, and was in Boulder last week. For myself I'm just about to leave for a few days of vacation and was inundated with a few deliverables for NASA HQ. Let's plan to touch base next Monday, which is currently completely open on my calendar.

>>> Cheers,

>>> - Tom

>>>

>>>

>>> On Jun 5, 2008, at 4:23 PM, Denis Gueyffier wrote:

>>>

>>> Hi Bill, Hi Tom,

>>> I will be glad to speak with you very soon about FVcore&the cubed sphere grid.

>>> Best,

>>> Denis Gueyffier

>>>

>>> On Thu, Jun 5, 2008 at 1:10 PM, Gavin Schmidt <gschmidt@giss.nasa.gov> wrote:

>>>>

>>>> Denis, I'm forwarding this email to Bill Putman and Tom Clune (they are
>>>> at GSFC and handle the interfaces). Bill in particular is the custodian
>>>> of the cubed sphere version and it is with him you will need to
>>>> coordinate.

>>>>

>>>> Bill, Tom, just to introduce Denis who will be working on the
>>>> FVcore/cubed sphere implementation here.

>>>>

>>>> Gavin

>>>>

>>>> On Thu, 2008-06-05 at 13:10, Denis Gueyffier wrote:

>>>> > Dear Max,

>>>>

>>>> > I'm a new researcher at GISS coming from NYU's Courant Institute. I am

• From: Sarah Galloway <galloway@amnh.org>
• Subject: Re: AMNH Video for Climate Change Exhibit
• Date: 06/25/08 12:55:22

Hi Sarah, the 25th is ok.

Gavin

On Mon, 2008-06-16 at 17:48, Sarah Galloway wrote:

> Thanks Gavin.

>

> So far that date works for several people.

>

> I'm planning on naming that date (Friday July 25th as well as July
> 24th) and hope that we can settle on them as two taping dates shortly.

>

> Sarah

>

>

> On 16 Jun, 2008, at 5:08 PM, Gavin Schmidt wrote:

>

> > july 25 is ok.

> >

> > Gavin

> >

> > On Mon, 2008-06-16 at 17:00, Sarah Galloway wrote:

> >> Hi Gavin,

> >>

> >> I just tried this number: 212/678-5627 and left a voice mail.

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> >> In general, we're aiming to tape our interviews later in July. If I
> >> had to pick a top date right now it would be July 25th, but we will
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> >> Would you be available July 25th or another date in July?

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> >> I'd like to hear from you in the next few days so I may plan our
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> >> taping days.

> >>

> >> Call me at 212/496-3461 or e-mail back.

> >>

> >> Best regards,

> >>

> >> Sarah

> >>

> >>

> >> Sarah Galloway, Senior Media Producer

> >> Exhibition Department

> >> The American Museum of Natural History

> >> Central Park West @ 79th Street

> >> New York, NY 10024-5192

> >> Tel: (212) 496-3461

> >> E-mail: galloway@amnh.org

> >>

> >> On 11 Jun, 2008, at 12:53 PM, Gavin Schmidt wrote:

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> >>> Sounds great. I'm glad to hear this is proceeding and I'd love to
> >>> help out. I'm out of town until next week, but call me then so that
> >>> we can discuss details.

> >>>

> >>> Gavin

> >>>

> >>> *-----

> >>> -*

> >>> | Gavin Schmidt NASA/Goddard Institute for Space Studies

> >>> |

> >>> | 2880 Broadway

> >>> |

> >>> | Tel: (212) 678 5627 New York, NY 10025

> >>> |

> >>> |

> >>> |

> >>> | gschmidt@giss.nasa.gov <http://www.giss.nasa.gov/~gavin>

> >>> |

> >>> *-----

> >>> -*

> >>>

> >>> On Wed, 11 Jun 2008, Sarah Galloway wrote:

> >>>

> >>>> Dear Dr. Gavin Schmidt,

> >>>>

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> >>>> scientists and other experts who will speak about clean energy
> >>>> technologies, government policy, economics, and the risks involved
> >>>> in facing climate change.

> >>>>

> >>>> As the video outlines the challenges that lie ahead, it also
> >>>> reminds us that we have successfully faced global challenges
> >>>> before. Human activity has been a contributing factor to climate
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> >>>> force in creating a more sustainable future.

> >>>>

> >>>> As your work in climate modeling and your outreach work with
> >>>> RealClimate.org represents a positive response to climate change,
> >>>> we would very much like to include your voice in this program, to
> >>>> help bring this message to our visitors.

> >>>>

> >>>> At this time I'd like to see if you have an interest in
> >>>> participating and are available for an on-camera interview for
> >>>> "Rethinking Our Energy Future". Video production will take place
> >>>> in July and early August of this year.

> >>>>

> >>>> I'd like to have an opportunity to discuss the project with you in

4/7/99 questions or concerns you may have. I'll follow up with a phone
> >>>> call shortly.
> >>>>
> >>>> Best regards,
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> >>>> Sarah
> >>>>
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> >>>> Sarah Galloway, Senior Media Producer
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> >>>> E-mail: galloway@amnh.org
> >>>> Web site: AMNH.org
> >>>>
> >>
> >>
>
>

To: Gavin A Schmidt <gschmidt@nccs.nasa.gov>

Subject: [Cubed Sphere Migration] New Comment: "Notes from kickoff meeting on 6/18/08"

Date: 06/25/08 11:05:50

Gavin A Schmidt,

A new comment was created on the blog post Notes from kickoff meeting on 6/18/08:

<https://modelingguru.nasa.gov/clearspace/blogs/cubedsphere/2008/06/25/notes-from-kickoff-mee>

Author : dgueyffi

Profile: <https://modelingguru.nasa.gov/clearspace/people/dgueyffi>

Comment:

Igor Aleinov was not present during the kickoff meeting. Maxwell Kelley was attending the meeting.

To: Gavin A Schmidt <gschmidt@nccs.nasa.gov>
Subject: [Cubed Sphere Migration] New Comment: "Notes from kickoff meeting on 6/18/08"
Date: 06/26/08 13:38:12

Gavin A Schmidt,

A new comment was created on the blog post Notes from kickoff meeting on 6/18/08:
<https://modelingguru.nasa.gov/clearspace/blogs/cubedsphere/2008/06/25/notes-from-kickoff-mee>

Author : dgueyffi

Profile: <https://modelingguru.nasa.gov/clearspace/people/dgueyffi>

Comment:

that's right. I also discussed this a little with Gary Russell and I believe that the time-averaged zenith angle COSZ is to be seen as a function of space (time has disappeared after the averaging) that we can sample at any point on the sphere, i.e. at lat-lon discrete points or at the cubed-sphere cell centers. See my post above on the scoping of RAD_DRV.f

Denis

To: mkelley@giss.nasa.gov, gschmidt@giss.nasa.gov

Subject: Re: FV core

Date: 06/25/08 12:38:03

Hi,

Tom and Bill have posted notes regarding the kickoff meeting at <https://modelingguru.nasa.gov/clearspace/blogs/cubedsphere>

Denis

On Wed, Jun 25, 2008 at 11:10 AM, Denis Gueyffier <dgueyffier@giss.nasa.gov> wrote:

Tom,

I've been able to post a first comment on the blog.

Thanks, Denis

On Wed, Jun 25, 2008 at 10:51 AM, Clune, Thomas L. (GSFC-610.3) <thomas.l.clune@nasa.gov> wrote:

OK - you should be good now. The admin for Guru is out today, though, so I can't yet make you an author for the blog. You should be able to "comment" on the posts though.

- Tom

On Jun 25, 2008, at 10:18 AM, Denis Gueyffier wrote:

Dear Tom,

Thanks.

I have applied but when I submitted the application a message on the screen was saying I already submitted an application before, so I assumed things were working fine.

I'm going to call the helpdesk at NCCS.

Best,

Denis

On Wed, Jun 25, 2008 at 10:12 AM, Clune, Thomas L. (GSFC-610.3) <thomas.l.clune@nasa.gov> wrote:

Hi Denis,

~~We don't see an ID for you yet on Guru. Have you applied for one online? I've created the blog for posting our meeting notes, but want to add you as an author.~~

On Jun 20, 2008, at 12:01 PM, Denis Gueyffier wrote:

Tom,

- > Yes - but there are some interesting wrinkles. First, anyone can _see_ most
- > of the content on Modeling Guru. An account is only needed to post. You
- > are free to go ahead and get an account on Modeling Guru, but when you get
- > an NCCS ID there is a risk you'll have 2 different Modeling Guru accounts
- > since the NCCS ID automatically generates a MG ID. Until now, that has not
- > been much of an issue since MG is new, and people already had NCCS ID's. I
- > presented the issue to the NCCS yesterday and we're working on a long term
- > strategy. In the meantime, since the consequence of having 2 ID's is
- > rather small, I recommend going ahead and getting your MG account.

Thanks. I just submitted an application online to get access to MG.

Best,

Denis

Thomas Clune, Ph.D. 301-286-4635 (W)
Advanced Software Technology Group 240-266-0400 (F)
Software Integration and Visualization Office <Thomas.L.Clune@nasa.gov>
NASA GSFC (610.3) <<http://sivo.gsfc.nasa.gov>>

Thomas Clune, Ph.D. 301-286-4635 (W)
Advanced Software Technology Group 240-266-0400 (F)
Software Integration and Visualization Office <Thomas.L.Clune@nasa.gov>
NASA GSFC (610.3) <<http://sivo.gsfc.nasa.gov>>

To: Gavin A Schmidt <gschmidt@nccs.nasa.gov>

Subject: [Cubed Sphere Migration] New Comment: "Notes from kickoff meeting on 6/18/08"

Date: 06/26/08 12:56:36

Gavin A Schmidt,

A new comment was created on the blog post Notes from kickoff meeting on 6/18/08:
<https://modelingguru.nasa.gov/clearspace/blogs/cubedsphere/2008/06/25/notes-from-kickoff-mee>

Author : clune

Profile: <https://modelingguru.nasa.gov/clearspace/people/clune>

Comment:

I'm certainly open to adding Reto (and others), but anyone should be able to post comments to threads. The real problem is simply how to let new people know to "look". Generally someone needs to send the person an email with a link to this blog and then they will be able to get email notifications for new posts. Since we are authors, we are not seeing the same options that non-authors see, but I think there will be an "email notifications" button for Reto if he logs in ...

To: Gavin A Schmidt <gschmidt@nccs.nasa.gov>

Subject: [Cubed Sphere Migration] New Comment: "Questions"

Date: 06/26/08 12:30:33

Gavin A Schmidt,

A new comment was created on the blog post Questions:

<https://modelingguru.nasa.gov/clearspace/blogs/cubedsphere/2008/06/26/questions#comments-1201>

Author : wputman

Profile: <https://modelingguru.nasa.gov/clearspace/people/wputman>

Comment:

Yes we are using the gnomonic grid, and those metric terms are used as in the JCP paper. Momentum is handled in a similar fashion.

The cube is unfolded as on the SIV0 page, and there are 3 ghost cells in each direction.

Yes, the area is computed using the spherical excess formula, and the core makes available several geometric factors including grid lengths, unit vectors, and the sin(alpha) and cos(alpha) metric factors.

The only other places where special treatment of the corners come in is the kinetic energy term in the D-grid SW equations, and the divergence damping. There is no detailed documentation on this as of yet, just what comments/code appear in sw_core.F90

To: Gavin A Schmidt <gschmidt@nccs.nasa.gov>

Subject: [Cubed Sphere Migration] New Comment: "problem using mpp_update_domains in the QUS"

Date: 06/29/08 12:25:10

Gavin A Schmidt,

A new comment was created on the blog post problem using mpp_update_domains in the QUS:
<https://modelingguru.nasa.gov/clearspace/blogs/cubedsphere/2008/06/27/problem-using-mppupdate>

Author : kelley

Profile: <https://modelingguru.nasa.gov/clearspace/people/kelley>

Comment:

I've replicated and adapted the relevant bits of code to create a "column" version. This will allow work to continue until a more elegant solution is found. Or, a less elegant solution, like some f77-style routines with extra arguments specifying which dimensions correspond to X and Y.

To: Gavin A Schmidt <gschmidt@nccs.nasa.gov>
Subject: [Cubed Sphere Migration] New Comment: "Scoping of rad_drv.f"
Date: 07/01/08 12:00:18

Gavin A Schmidt,

A new comment was created on the blog post Scoping of rad_drv.f:
<https://modelingguru.nasa.gov/clearspace/blogs/cubedsphere/2008/06/28/scoping-of-raddrvf#comment>

Author : dgueyffi

Profile: <https://modelingguru.nasa.gov/clearspace/people/dgueyffi>

Comment:

Thanks. Yes, this is what I meant. Do you have plans for using a similar data structure in the FVcubed library?

To: Gavin A Schmidt <gschmidt@nccs.nasa.gov>
Subject: [Cubed Sphere Migration] New Comment: "Compiling the FVcubed dynamical core"
Date: 07/03/08 10:12:42

Gavin A Schmidt,

A new comment was created on the blog post Compiling the FVcubed dynamical core:
<https://modelingguru.nasa.gov/clearspace/blogs/cubedsphere/2008/07/02/compiling-the-fvcubed-1>

Author : wputman
Profile: <https://modelingguru.nasa.gov/clearspace/people/wputman>

Comment:

You won't be able to run FVcubed standalone outside of the GEOS framework with that tarball from the dycore colloquium. It is setup to run dycore only withing the GEOS framework. If you really want to try the dycore only within GEOS on your Mac, you'll need to port baselibs (a set of external utilities GEOS needs), and you'll likely need an intel compiler, I don't know about the support of baselibs for the g95 compiler, the cubed-sphere has not been ported to that compiler yet.

If you'd like to continue exploring this, the file you are looking for should be in FVCubed_dycore_GridComp/fvcubed_dycore/shared/mpp/include/fms_platform.h of the tarball. You'll need to update the mkmf.template as you have done, and point to the correct one in mk_fvcore.

You need baselibs built on your Mac, which I am working on now with my intel Mac and intel compilers.

Then you'll need to edit the Config/ESMA_arch.mk file to add a section to support your architecture and compiler, etc...

Building by simply executing, gmake install, in the src directory.

To: Gavin A Schmidt <gschmidt@nccs.nasa.gov>

Subject: [Cubed Sphere Migration] New Comment: "Compiling the FVcubed dynamical core"

Date: 07/04/08 16:30:52

Gavin A Schmidt,

A new comment was created on the blog post Compiling the FVcubed dynamical core:

<https://modelingguru.nasa.gov/clearspace/blogs/cubedsphere/2008/07/02/compiling-the-fvcubed->

Author : dgueyffi

Profile: <https://modelingguru.nasa.gov/clearspace/people/dgueyffi>

Comment:

Below are some notes about the installation of the FVcubed package on my 64-bit Mac. It can be useful to others. I will add them to the CVS when we will set it up.

- * install Xcode
- * install ifort
- * get openmpi tarball and untar in /usr/local (to avoid confusion with stuff that could be in /sw/)
- * configure openmpi using command
 sudo ./configure F77=ifort FC=ifort CFLAGS=-m64 CXXFLAGS=-m64
- * compile openmpi using
 sudo make all install
- * make sure that the new mpif90 is in your PATH
- * get netcdf tarball and untar in /usr/local
- * define ifort environment variables using
 source /opt/intel/fce/10.1.014/bin/ifortvars.sh
- * configure netcdf using
 sudo ./configure F77=ifort FC=ifort LDFLAGS=-lSystemStubs --enable-f90 CFLAGS=-m64 CXXFLAGS=-m64

To: Gavin A Schmidt <gschmidt@nccs.nasa.gov>
Subject: [Cubed Sphere Migration] New Comment: "plans for a branch?"
Date: 07/11/08 13:55:25

Gavin A Schmidt,

A new comment was created on the blog post plans for a branch?:
<https://modelingguru.nasa.gov/clearspace/blogs/cubedsphere/2008/07/11/plans-for-a-branch#comment>

Author : clune
Profile: <https://modelingguru.nasa.gov/clearspace/people/clune>

Comment:

Branches in CVS are scary business, but you are right that it might become necessary. My strong preference is to work just a bit harder and identify incremental changes that can be integrated on the trunk. Often this means building up duplicate infrastructure rather than replacing the original right away. Then you delete the original infrastructure once nothing relies upon it.

One of the most impressive things about modelE is the continuous integration. I'd hate to lose that capability.

Reply-To: V. Balaji <V.Balaji@noaa.gov>

To: Gavin Schmidt <gschmidt@giss.nasa.gov>

Subject: Re: pre-AR5 workshop at GISS: suggestions for attendees?

Date: 07/14/08 23:07:31

Gavin, great to see this going ahead!

There seems to be no overseas participation, is this by design, funding constraints, etc? It might be good to see reps from Hadley, Max Planck, etc. Esp since several of them are working on probabilistic prediction, weighting, and so on. There was that whole issue of Phil Trans about it last year. James Murphy, Matt Collins, Sylvia Knight, Annan and Hargreaves. Also Myles Allen, if you want to go there.

Second, I find WG2 concerns to be under-represented! This seems always to be the case when we WG1 types get together. Perhaps Cynthia could attend, talk about the technical issues behind her mega-comprehensive impacts paper? It's vital we get them to the table, AR5 is going to be mostly about impacts.

On a more perso note... I'd like to demonstrate some complex analyses that you can deliver for these datasets. We've long been talking about developing some of the things you mentioned in your RC critique of the CMPI3 database, and I took on the specific challenge you laid down there, "NINO SS3 spectra" as an analysis on any model in the database. Can demo that and steps toward making that sort of thing a reality: where the user is in one place, the data in a second, and the analysis in a third.

I thought about sending this to your email list, but decided not to turn that into a discussion thread. If that would be your preference, I (or you) could post this to the list.

Thanks, let me know if there's anything else I can do to help.

Gavin Schmidt writes:

> Topics that we hope to cover:

- > - assessment of what worked in CMIP3 and what was missing
- > - Improving documentation/access/coordination
- > - Tests of the new paradigms for AR5 runs/candidate model testing
- > - Tapping into the CMIP3 expertise for model development purposes
- > - progress using super-ensembles and weighting schemes
- > - Novel data-distribution/Web2.0 ideas

--

V. Balaji
Head, Modeling Systems Group, GFDL
Princeton University

Office: +1-609-452-6516
Home: +1-212-253-6662
Email: v.balaji@noaa.gov

To: Gavin A Schmidt <gschmidt@nccs.nasa.gov>

Subject: [Cubed Sphere Migration] New Comment: "Notes from July 17 meeting"

Date: 07/17/08 14:28:35

Gavin A Schmidt,

A new comment was created on the blog post Notes from July 17 meeting:

<https://modelingguru.nasa.gov/clearspace/blogs/cubedsphere/2008/07/17/notes-from-july-17-mee>

Author : wputman

Profile: <https://modelingguru.nasa.gov/clearspace/people/wputman>

Comment:

Note that the exported mass fluxes from FVcubed (via ESMF) are already remapped from the Lagrangian to Eulerian vertical coordinate. This will be slightly different from the current implementation in the QUS that Max described which was using the un-remapped fluxes on Lagrangian surfaces.

To: Gavin A Schmidt <gschmidt@nccs.nasa.gov>

Subject: [Cubed Sphere Migration] New Comment: "Replacing DOMAIN_DECOMP gather/scatter with calls to MPP"

Date: 07/18/08 16:19:48

Gavin A Schmidt,

A new comment was created on the blog post Replacing DOMAIN_DECOMP gather/scatter with calls to MPP:

<https://modelingguru.nasa.gov/clearspace/blogs/cubedsphere/2008/07/18/replacing-domaindecomp>

Author : kelley

Profile: <https://modelingguru.nasa.gov/clearspace/people/kelley>

Comment:

I don't mean to suggest that we try to avoid making any gather routines available to the model.

But I will note that the gathers for scaling of diagnostics can be avoided if the I/O problem is somehow solved without us having to explicitly use a gather routine. The diagnostic accumulations on each processor can be written to a global file and then read back in by the processor(s) responsible for doing the scaling (and regridding). This should only be necessary for the lat-lon and lat-lon-height diagnostics which require regridding. Assuming that no regridding is necessary for zonal-average accumulations, globalsums can be used instead of gathers.

Using the disk as a primitive kind of read-only shared memory would also make it more straightforward to compute eddy and spectral statistics (from temporary history files). This could be done automatically at the end of each month to ensure that no postprocessing steps are required of the user.

To: Gavin A Schmidt <gschmidt@nccs.nasa.gov>

Subject: [Cubed Sphere Migration] New Comment: "Replacing DOMAIN_DECOMP gather/scatter with calls to MPP"

Date: 07/21/08 13:18:47

Gavin A Schmidt,

A new comment was created on the blog post Replacing DOMAIN_DECOMP gather/scatter with calls to MPP:

<https://modelingguru.nasa.gov/clearspace/blogs/cubedsphere/2008/07/18/replacing-domaindecomp>

Author : ialeinov

Profile: <https://modelingguru.nasa.gov/clearspace/people/ialeinov>

Comment:

We could use a UNIX "mmap" call. It maps file into a memory without reading it. The portions of file are read only when corresponding memory is accessed. So one gets a kind of virtual shared memory setup. At least for reading this should work fine.

To: NATALIE KEHRWALD <kehrwald.1@osu.edu>
Subject: Re: proxy data
Date: 07/29/08 16:01:57

Hi there! Nice to hear from you.

On Tue, 2008-07-29 at 14:37, NATALIE KEHRWALD wrote:

> Gavin:

>

> I heard from a good friend of mine, Branwen Williams, that you were at
> a conference/workshop in Italy where one of the topics was how to
> integrate proxy data with climate models. I have been wondering about
> this lately as I have completed my lab analyses and am using the
> results to write papers and is also something I would like to keep in
> mind as I continue my career. What are the ideal aspects of data that
> would help modelers? Is there any information you feel is currently
> missing or unavailable to the general scientific community? How could
> the data be presented (parameters, ages, etc.) to help with model
> development or paleoclimate interpretation? I realize these are not
> short answer questions, but I would appreciate any answer of any
> length.

I'm actually supposed to be writing a paper on this - so you could wait for that, but basically what modelers need are good hypotheses to test and good sets of data to compare against. The principle part that's missing is the synthesis of data sets - which is hard, particularly with the way data centers are set up and the limited amount of meta data that gets archived. For instance when was the last time you saw a gridded map of ocean core data at any time slice apart from the LGM?

> I am embarrassed about being a bit of a deadbeat when I was at GISS
> mainly for not really realizing what was possible as far as
> integrating the data I had with the model. I was wondering if you
> think any work can be salvaged from that summer or if you think it
> should be chalked up to a learning experience?

I think we could salvage something - You did the correlations of you core location 018 to regional patterns right? and I've done it the other way around (Schmidt et al, 2007). We currently have a summer visitor from Mike Gagan's lab (ANU) and she is analysing water sources for key locations - I can get here to do yours as well. That would make a short paper informing core interpretations - even if it doesn't perfectly match with 20th data.

> (Ie I learned I am very much a data person although I do believe that
> people need to change with the times and therefore learn how to better
> use their data).

:)

> Another part of being a data person is that I am completely hooked on
> field work. I just came back from Peru with Lonnie doing the usual
> investigations on Quelccaya and checking out a new drilling site on
> Hualcan. If you drew a cartoon of a mountain for ice core drilling,
> this looks something like that, with steep sides and a huge plateau on
> the top. Ray Bradley was there too on vacation with his son, so it was
> fun to see them in the field. One of my primary jobs was to translate
> in order to facilitate the permit-getting process and interviews, and

> anything in your research prove the existence of the lost city of
> Atlantis?"

that's hilarious. Do you want to write a little travelogue for
RealClimate?

> --- I will be in Antarctica for three months this winter creating the
> electrical conductivity measurements for the WAIS Divide core which I
> am really looking forward to but which really gets in the way of the
> finishing-up process. (I guess on the EPICA core, there was some
> problems with their DEP analyses where they initially ended up missing
> an entire glacial cycle. I hope this is not the case for me or there
> will be some messed up models!)

That'll be hard work - I've talked to some of the people at the inland
stations before.... but worth it!

Gavin

> Best,
> Natalie
>
> Natalie Kehrwald
> Byrd Polar Research Center
> Ohio State University
> 1090 Carmack Road
> Columbus, OH 43210

To: Gavin A Schmidt <gschmidt@nccs.nasa.gov>
Subject: [Cubed Sphere Migration] New Comment: "Needs in terms of domain decomposition (1st draft)"
Date: 07/31/08 10:39:05

Gavin A Schmidt,

A new comment was created on the blog post Needs in terms of domain decomposition (1st draft):
<https://modelingguru.nasa.gov/clearspace/blogs/cubedsphere/2008/07/29/needs-in-terms-of-domain-decomposition>

Author : clune

Profile: <https://modelingguru.nasa.gov/clearspace/people/clune>

Comment:

I think that we should keep our sight on ESMF_GRID, since we have a long term goal to be compatible with other ESMF applications. What that means in practice is quite flexible. In the short term we will be kludging a lat-lon ESMF_GRID to represent the cubed-sphere, but true support for the cubed-sphere within ESMF should be emerging over the next year. (And some is already present, but not halo operations and such.) Most of the code does not need to know anything about grid, so an argument to remove the ESMF_GRID component from the GRID datatype could easily be made.

To: gschmidt@giss.nasa.gov

Subject: salvage

Date: 07/31/08 12:38:50

Gavn,

Great! It would be wonderful to salvage something (below). What information do you need from me? I could send it to you and to her -- I am at a conference in OK right now but have most of this information with me on my laptop. Let me know what you think would be helpful and any background information that could augment the data and plots.

"I think we could salvage something - You did the correlations of you core location O18 to regional patterns right? and I've done it the other way around (Schmidt et al, 2007). We currently have a summer visitor from Mike Gagan's lab (ANU) and she is analysing water sources for key locations - I can get here to do yours as well. That would make a short paper informing core interpretations - even if it doesn't perfectly match with 20th data."

"that's hilarious. Do you want to write a little travelogue for RealClimate?"

Were you serious about this? If so, I would love to do it!
Natalie

Natalie Kehrwald
Byrd Polar Research Center
Ohio State University
1090 Carmack Road
Columbus, OH 43210

To: gschmidt@giss.nasa.gov

Cc: Ben Santer <santer1@llnl.gov>

Subject: Request to participate in NASA proposal

Date: 08/29/08 12:27:29

Dear Dr. Schmidt,

I am contacting you at the suggestion of Ben Santer (who happens to be my brother), regarding the NASA Research Announcement "Competitive Program for Science Museums and Planetariums."

I work at the Miami Science Museum, and the issue of global climate change has been one of our key areas of interest, particularly given our low-lying coastal location and some of the exciting weather events we are prone to!

Another area of interest to us has been the exploration of emerging virtual world environments, like Second Life, and the extent to which these can be used to help convey challenging science concepts to our visitors.

Over the past year or so, we have been collaborating with a group of education researchers and with scientists at NOAA/ESRL, to design and test some virtual world applications themed around climate change, to increase our understanding of how best to design and support these kinds of virtual world learning experiences. (As you may know, NOAA along with NASA have taken a lead in exploring how science can be presented to the public in virtual environments, and have an impressive set of virtual world exhibits already in Second Life.)

We have one proposal pending with NSF, but a wonderful opportunity has just presented itself to submit a modified version of this effort to NASA, under the above-referenced NRA, which specifically targets museums and planetariums.

The NRA requires a link with one or more NASA Mission Directorates and Centers, and allows funding from the grant to be directed to support all such NASA participation.

I know Ben does a lot of work with NASA, so I asked him for some leads, and he suggested I contact you. Ben was kind enough to provide us with some input on our prior proposal effort, so he has a pretty good idea of what we're trying to do, and thought there might be some alignment with your interests, both in terms of the focus of your research as well as the RealClimate web site you maintain.

If you have the time, I would love the opportunity to tell you more about our proposed project, and see whether you might be able to assist us.

Thanks very much,

Jennifer Santer
Senior Director
Miami Science Museum
(305) 284-2744
www.miamisci.org

To: Raimund Muscheler <Raimund.Muscheler@geol.lu.se>
Subject: Re: Lund university climate workshop
Date: 08/29/08 11:12:21

Hi Raimund,

Sorry but I'm running a workshop myself those dates and so I can't come. But a few thoughts you may find relevant. First, don't fall into the trap of trying to be 'fair' and hear all sides- they will abuse your sense of fair play to just spread disinformation. You are likely to get significant coverage among the 'climate sceptic underground' and the spin will be that they are making ground on the consensus regardless of how effectively the nonsense is debunked.

Second, I fail to see why Svensmark is continues to be given any credibility at all - he has a proven record of data manipulation, exaggerated claims and ridiculous extrapolation. 'The Chilling Stars' should have been the end to any pretensions of serious scholarship. He was even chucked off the PI team for the CERN CLOUD project! His continued role as the GCR-climate spokesman discredits the whole enterprise. Ask Jason Kirkby if you want someone possibly worth talking to. Or Frohlich, or Lockwood. Or Peter Laut (he's retired though).

Stilbs is a joker and not at all serious on this issue. Stigebrandt I don't know.

If you want a debunker, Rasmus Benestad is reasonable and relatively local. Eystein Jansen is pretty good too - though not necessarily what you might need for dealing with the skeptics. Reto is a good choice. Stefan Rahmstorf is also very experienced in these matters.

Gavin

On Fri, 2008-08-29 at 08:58, Raimund Muscheler wrote:

> Hej Gavin,
>
> how are you? I hope everything is fine.
>
> I am writing you because we would like to invite
> you to come to Lund for a workshop about climate
> uncertainties and climate prediction (Nov 19-20).
> We would pay for your flight and accommodation.
>
> A bit about the background: The rector of Lund
> university started a climate initiative with the
> goals to increase the knowledge about the causes
> for climate change and how to mitigate climate
> change. Within this initiative we got funding for
> a workshop with the title: "Human influence on
> climate - is it beyond any doubt?"
>
> The idea behind this workshop is the following.
> As you know climate sceptics get (unfortunately)
> too much attention in the media (with too much
> impact on the public opinion) and I think it
> would be good to confront these people with solid
> arguments. I am planning to invite people like

> e.g. how the sun could have influence the climate
> during the last 50 years. It should be an open
> discussion about certainties and uncertainties in
> climate research.
>
> I think you you would be the perfect person to
> have in this discussion. You could also present
> your model results and/or the idea behind
> realclimate.org and how this was perceived by the
> public.
>
> Here is the list of other invited speakers that
> we suggested (but I am only starting to contact
> the people):
>
> Dorte Dahl-Jenssen, Centre for Ice and Climate,
> Denmark; Natural climate change
> reconstructed from ice cores
> Reto Knutti, ETH Zurich, Switzerland; Uncertainties in climate predictions
> Anders Moberg, Department of Physical Geography and Quaternary Geology.
> Stockholm, Climate during the last Millennium
> Anders Stigebrandt, Earth Science Center, Gothenburg University, Is the climate
> threat exaggerated?
> Peter Stilbs - Physical Chemistry / KTH Stockholm, Potential errors in the IPCC
> report
> Henrik Svensmark - Danish National Space Center, Copenhagen, The Sun as a
> major climate-forcing factor
>
> There will be another workshop: "Carbon cycling
> and its interactions with the climate system"
> just before our workshop. If you are interested
> you could come two days earlier and attend this
> one, too.
>
> What do you think? Do you have time and do you
> think that this could be interesting for you? We
> would be happy if you could come.
>
> Best wishes,
> Raimund
>

To: gschmidt@giss.nasa.gov

Cc: Ben Santer <santer1@llnl.gov>

Subject: Request to participate in NASA proposal

Date: 08/29/08 12:27:29

Dear Dr. Schmidt,

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I work at the Miami Science Museum, and the issue of global climate change has been one of our key areas of interest, particularly given our low-lying coastal location and some of the exciting weather events we are prone to!

Another area of interest to us has been the exploration of emerging virtual world environments, like Second Life, and the extent to which these can be used to help convey challenging science concepts to our visitors.

Over the past year or so, we have been collaborating with a group of education researchers and with scientists at NOAA/ESRL, to design and test some virtual world applications themed around climate change, to increase our understanding of how best to design and support these kinds of virtual world learning experiences. (As you may know, NOAA along with NASA have taken a lead in exploring how science can be presented to the public in virtual environments, and have an impressive set of virtual world exhibits already in Second Life.)

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I know Ben does a lot of work with NASA, so I asked him for some leads, and he suggested I contact you. Ben was kind enough to provide us with some input on our prior proposal effort, so he has a pretty good idea of what we're trying to do, and thought there might be some alignment with your interests, both in terms of the focus of your research as well as the RealClimate web site you maintain.

If you have the time, I would love the opportunity to tell you more about our proposed project, and see whether you might be able to assist us.

Thanks very much,

Jennifer Santer
Senior Director
Miami Science Museum
(305) 284-2744

www.miamisci.org

To: gschmidt@giss.nasa.gov
Subject: Re: Request to participate in NASA proposal
Date: 09/03/08 11:21:58

Hi, Dr. Schmidt,

I thought it might be helpful to send you this one-page summary of our project idea, in advance of my calling you this afternoon.

Regards,

Jennifer

On 9/3/08 9:42 AM, "Gavin Schmidt" <gschmidt@giss.nasa.gov> wrote:

>
> Hi Jennifer, sorry for the delay replying. I'm nominally well-disposed to
> your request, but obviously we'd need to discuss what you actually need
> before I can commit to anything. Feel free to give me a call this
> afternoon sometime (after 2pm).

> gavin

> *-----*
> | Gavin Schmidt NASA/Goddard Institute for Space Studies |
> | 2880 Broadway |
> | Tel: (212) 678 5627 New York, NY 10025 |
> | |
> | gschmidt@giss.nasa.gov <http://www.giss.nasa.gov/~gavin> |
> *-----*

> On Wed, 3 Sep 2008, Jennifer Santer wrote:

>> Dear Dr. Schmidt,

>> Thought I'd try sending this one more time, just in case the first time did
>> not go through.

>> Sincerely,

>> Jennifer Santer

>> ----- Forwarded Message

>> From: Jennifer Santer <jsanter@miamisci.org>

>> Date: Fri, 29 Aug 2008 12:27:29 -0400

>> To: <gschmidt@giss.nasa.gov>

>> Cc: Ben Santer <santer1@llnl.gov>

>> Subject: Request to participate in NASA proposal

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>>
>> We have one proposal pending with NSF, but a wonderful opportunity has just
>> presented itself to submit a modified version of this effort to NASA, under
>> the above-referenced NRA, which specifically targets museums and
>> planetariums.
>>
>> The NRA requires a link with one or more NASA Mission Directorates and
>> Centers, and allows funding from the grant to be directed to support all
>> such NASA participation.
>>
>> I know Ben does a lot of work with NASA, so I asked him for some leads, and
>> he suggested I contact you. Ben was kind enough to provide us with some
>> input on our prior proposal effort, so he has a pretty good idea of what
>> we're trying to do, and thought there might be some alignment with your
>> interests, both in terms of the focus of your research as well as the
>> RealClimate web site you maintain.
>>
>> If you have the time, I would love the opportunity to tell you more about
>> our proposed project, and see whether you might be able to assist us.
>>
>> Thanks very much,
>>
>> Jennifer Santer
>> Senior Director
>> Miami Science Museum
>> (305) 284-2744
>> www.miamisci.org
>>
>>
>> ----- End of Forwarded Message
>>
>>
>>

Word document attachment (summary for Gavin)

To: Stefan Rahmstorf <rahmstorf@ozean-klima.de>

Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

Subject: Re: The solar physics behind a "coming Maunder minimum"?

Date: 09/04/08 09:30:44

Stefan--that's a really cool idea. We could to a 21st century anthropogenic run where we initialize solar forcing to have its 1600 value now, and then it turns through a new MM about 10 years in lasting a half century or so. We would need to explore the sensitivity to the solar scaling. We could look at three different possible scalings, i.e. Lean old/Lean new

Gavin--what are your thoughts? How easy to get a coupled ModelE run going w/ this?

mike

On Sep 4, 2008, at 5:15 AM, Stefan Rahmstorf wrote:

I'd say this skeptics argument is likely going to be with us for some years, so perhaps it is worth doing not just a post, but a model simulation.

Gavin, what about GISS? We could easily do it with our model - we have done simulations for the past millennium, as shown in the AR4, and one could easily do a hypothetical Maunder minimum forcing happening in the coming decades alongside rising GHG and publish it in a journal. What it would show, of course, is that the reduction in insolation would be overwhelmed by the GHG.

Stefan

--

Stefan Rahmstorf

www.ozean-klima.de

www.realclimate.org

--
Michael E. Mann
Associate Professor
Director, Earth System Science Center (ESSC)

Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: mann@psu.edu
University Park, PA 16802-5013

website: <http://www.met.psu.edu/dept/faculty/mann.htm>

"Dire Predictions" book site: <http://www.pearsonhighered.com/academic/product/0,3110,0136044352,00.html>

To: Gavin Schmidt <gschmidt@giss.nasa.gov>
Subject: letter suggestions
Date: 09/29/08 01:41:27

Below are letter suggestions, in fulsome bad prose that must be heavily edited by Gavin. See Terry Barker and Rachel Warren letters (attached) for some models, though best to follow your own muse. Can you possibly make it first priority Monday AM to get something onto letterhead for us? I should have gotten this to you earlier during the weekend and that is my fault. Have been desperately working on other proposal-related stuff all evening.

Dear Liz (or "To Whom it may concern")

I am writing to express my enthusiasm for the proposal
"CIM-Earth: A Community Integrated Model of Economic and Resource
Trajectories for Humankind" and my willingness to collaborate on
resulting work.

I am a climate modeler at Goddard Institute of Space Science (GISS) and have been working on both coding of the GISS climate model and analysis of results, with an especial focus on statistics of climate outcomes and understanding ranges of possible forecasts. I have also served for many years as an advisor on climate science and climate policy for journalists, policymakers, and industry and citizen groups. (examples here.. In 200* I briefed Al Gore.. in 200* I spoke to various Masters of the Universe). My outreach efforts have increased since I founded the website realclimate.org in 200* to provide a neutral source of scientific information to the public; the site now receives *** hits per year. (cite any other indications of your vast importance, or your network of collaborations).

In the course of speaking on this issue to many parties over the last several years have grown increasingly aware of the limitations of policy analysis tools. Compared to our understanding of climate itself, the state of the art of understanding of impacts of climate change or even of cost of preventing it is primitive.

Interest in the issue led to my inviting you to collaborate on op-ed piece I was asked to write for Nature discussing difficulty of bringing climate scientists and economists together on this issue, despite its importance.

I strongly support efforts to advance the field of integrated assessment modeling, and especially to push it forward with a significant single effort rather than incremental adjustments. Field is in need of transformation. I would be very interested in participating in the planning process for such a project and in collaborating with a resulting modeling effort. I believe the interest level in collaboration would be strong in the climate community in general and that this effort would be very welcome and extremely well received. I have been involved with the CCSM modeling team and believe that this model would serve the policy analysis community well, also that physical scientists will be supportive of an effort as open and transparent and inclusive as the one proposed here.

(** maybe something like..) Finally, I am encouraged by the CIM-EARTH

community effort that is needed ..Have been encouraged in talking to your team that you are thinking sensibly about means of coupling climate information into economic models, i.e. focusing effort on considering distributions of climate outcomes, which can be run by off-line climate models. too many existing efforts do not think carefully about where computational resources are needed and so limit the utility of their output.

blah blah excited about opportunities to make advances on this critical issue blah blah

Gavin Schmidt

--

Dept. of the Geophysical Sciences
University of Chicago
5734 S. Ellis Ave.
Chicago, IL 60637
773.834.2992

PDF document attachment (Barker_letter.pdf)
PDF document attachment (Warren_letter.pdf)

10. James Annan <jannan@jamstec.go.jp>, Gavin Schmidt <gschmidt@gsi.nasa.gov>, Grant Foster <grant.foster@jamstec.go.jp>

Subject: Re: [Fwd: Publication Notice, 2007JD009373]

Date: 10/02/08 18:26:28

sure, I don't see why not. should be authored by the group of us.

Gavin--perhaps you get a rough draft up for the rest of us to comment on/add to?

mike

On Oct 2, 2008, at 6:15 PM, James Annan wrote:

Slow time on RC?

I'm game.

BTW I fixed your mail :-)

James

Gavin Schmidt wrote:

Did we want to draw attention to this on RC and have a little editorial on the pointLESSNESS of it all?

Gavin

On Sat, 2008-08-02 at 09:38, James Annan wrote:

Finally...

James

----- Original Message -----

Subject: Publication Notice, 2007JD009373

Date: Sat, 2 Aug 2008 05:15 -0400

From: DoNotReply@agu.org

To: [jdannan@jamstec.go.jp](mailto:jannan@jamstec.go.jp)

Congratulations! Your article, "Comment on "Heat capacity, time constant, and sensitivity of Earth's climate system" by S. E. Schwartz", was published today in Journal of Geophysical Research.

The complete citation is

Foster, G., J. D. Annan, G. A. Schmidt, and M. E. Mann (2008), Comment on "Heat capacity, time constant, and sensitivity of Earth's climate system" by S. E. Schwartz, J. Geophys. Res., 113, D15102, doi:10.1029/2007JD009373.

MAKE IT COUNT: Use this complete AGU citation wherever you publish to increase the likelihood that citations to your article are counted.

We recognize that reference styles vary by journal, but certain elements, most notably, the citation number ("D15102" above), must be

When publishing in AGU journals, you can rely on copy editors to make sure that reference citations are complete. When publishing in other journals, however, it is your responsibility to provide a complete reference and to ensure that critical elements including the citation number are retained.

For your convenience and the convenience of others wishing to cite your article, the complete citation appears here and in both online and print articles. Additional information on How to Cite AGU Articles is available at <http://www.agu.org/pubs/citing.html>

--
James D Annan jdannan@jamstec.go.jp Tel: +81-45-778-5618 (Fax 5707)
Senior Scientist, Frontier Research Centre for Global Change, JAMSTEC
3173-25 Showamachi, Kanazawa-ku, Yokohama City, Kanagawa, 236-0001 Japan
<http://www.jamstec.go.jp/frcgc/research/d5/jdannan/>

--
Michael E. Mann
Associate Professor
Director, Earth System Science Center (ESSC)

Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: mann@psu.edu
University Park, PA 16802-5013

website: <http://www.meteo.psu.edu/~mann/Mann/index.html>

"Dire Predictions" book site:

http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Reply-to: santer1@llnl.gov

To: Gavin Schmidt <gschmidt@giss.nasa.gov>

Subject: Re: Changes

Date: 10/02/08 16:54:08

Dear Gavin,

Sure! That's fine by me. I've spent all morning on the press release, and now have some time to work on the fact sheet. I'll send you the next draft of the fact sheet later this afternoon.

With best regards,

Ben

Gavin Schmidt wrote:

> PS. It kind of goes without saying that I'll post the Fact Sheet (when
> that's ready) directly on RC - I hope that's ok.

>

> On Thu, 2008-10-02 at 15:28, Ben Santer wrote:

>> Dear Gavin and Peter,

>>

>> Here are the changes that I've made (tracked in green) in response to
>> your most recent comments. Are we good to go?

>>

>> Best regards,

>>

>> Ben

>>

>> Benjamin D. Santer

>> Program for Climate Model Diagnosis and Intercomparison

>> Lawrence Livermore National Laboratory

>> P.O. Box 808, Mail Stop L-103

>> Livermore, CA 94550, U.S.A.

>> Tel: (925) 422-3840

>> FAX: (925) 422-7675

>> email: santer1@llnl.gov

>>

>>

>

--

Benjamin D. Santer

Program for Climate Model Diagnosis and Intercomparison

Lawrence Livermore National Laboratory

P.O. Box 808, Mail Stop L-103

Livermore, CA 94550, U.S.A.

Tel: (925) 422-3840

FAX: (925) 422-7675

email: santer1@llnl.gov

10. Pratigya Polissar <[REDACTED]>
Subject: Re: ModelE Isotopes

Date: 10/09/08 17:12:45

Pratigya,

Most of the that data (for the 20th C) can be downloaded from the SWING database (<http://atoc.colorado.edu/~dcn/SWING/index.php>) along output from 2 other models. I suggest you start off with that. We have done more paleo simulations, and future warming simulations as well, and with a little work we could make those available also.

The contact for the extra data (perhaps once you've done a basic sanity check on the modern values!) would be Allegra LeGrande (legrande@giss.nasa.gov).

Thanks for your interest in our work,

Gavin

On Thu, 2008-10-09 at 16:53, Pratigya Polissar wrote:

> Dear Dr. Schmidt,

>

> I am writing to inquire about the availability of data from GISS
> ModelE runs that included isotope tracers. What follows is a little
> background on myself and my interest in this data.

>

> I am a post-doctoral researcher with Kate Freeman at Penn State
> University and Jim Zachos at UC-Santa Cruz. My current research is
> focused on past hydrologic variability reconstructed from the D/H
> ratio of plant-wax compounds. As part of this research I have
> studied modern plants and sediments to improve our understanding of
> the controls on plant-wax dD. This and other studies indicate that
> precipitation dD, soil evaporation and leaf transpiration are all
> determinants of plant-wax dD. This study has led me to attempt to
> model these processes and predict what the dD of plant waxes would
> look like under different climate scenarios. What I would really
> like to do is take a global dataset of climate and precipitation
> isotope data and calculate theoretical values for plant wax dD. Even
> better would be to do this calculation for both modern climate and
> future warming scenarios. Jim and I are currently submitting a
> proposal (P2C2) to generate records of plant-wax dD through the PETM
> and ELMO events at sites from different latitudes. It would be
> incredibly valuable for this proposal if we could make some
> quantitative predictions using modern/future data for the pattern and
> magnitude of plant-wax dD changes we would expect in different
> regions.

>

> So, I wonder whether any of the ModelE runs would be appropriate for
> this goal (particularly those presented in Aleinov & Schmidt, 2006
> that included soil evaporation/enrichment)? If so, is there any way
> that I could have access to output from the model? I am not trying
> to encroach on anyones research and would love for a more
> collaborative approach to this question, but am also happy
> calculating the plant-wax dD myself from model fields (Precip. Evap.
> surface relative humidity, precipitation dD, ?soil dD?) and giving
> credit or co-authorship wherever appropriate.

>

> Pratigya

>

> P.S. I have been an avid reader of RealClimate since its inception
> (R. Bradley was my Ph.D. advisor) and have greatly enjoyed your posts!

To: santer1@llnl.gov
Subject: Re: End of the road...
Date: 10/10/08 12:50:12

Ok. It is at

http://www.realclimate.org/docs/santer_etal_IJoC_08_fact_sheet.pdf

if anyone else needs it. I'll put up a post this afternoon or tomorrow.

Thanks

Gavin

On Fri, 2008-10-10 at 12:32, Ben Santer wrote:

> Dear Gavin,

>

> I don't think so. Feel free to post it on Real Climate if you think that
> would be appropriate.

>

> With best regards,

>

> Ben

>

> Gavin Schmidt wrote:

> > ben, is the fact sheet online anywhere?

> >

> > Gavin

> >

> > On Fri, 2008-10-10 at 12:21, Ben Santer wrote:

> >> Dear folks,

> >>

> >> Our paper was published online today. In the immortal words of Homer
> >> Simpson, "WooHoo!" I'm appending the pdf file, together with the final
> >> versions of the press release and fact sheet.

> >>

> >> Once again, many thanks for all your help, patience, and encouragement
> >> over the past 10 months.

> >>

> >> With best regards,

> >>

> >> Ben

> >>

> >> Benjamin D. Santer

> >> Program for Climate Model Diagnosis and Intercomparison

> >> Lawrence Livermore National Laboratory

> >> P.O. Box 808, Mail Stop L-103

> >> Livermore, CA 94550, U.S.A.

> >> Tel: (925) 422-3840

> >> FAX: (925) 422-7675

> >> email: santer1@llnl.gov

> >>

> >>

> >>

>

Reply-to: santner1@iuii.gov

To: Cawley Gavin Dr (CMP) <G.Cawley@uea.ac.uk>

Cc: 'Philip D. Jones' <p.jones@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Thorne, Peter <peter.thorne@metoffice.gov.uk>, Tom Wigley <wigley@cgd.ucar.edu>

Subject: Re: Possible error in recent IJC paper

Date: 10/31/08 00:06:31

Dear Gavin,

Thanks very much for your email, and for your interest in our recent paper in the International Journal of Climatology (IJoC). There is no error in equation (12) in our IJoC paper. Let me try to answer the questions that you posed.

The first term under the square root in our equation (12) is a standard estimate of the variance of a sample mean - see, e.g., "Statistical Analysis in Climate Research", by Francis Zwiers and Hans von Storch, Cambridge University Press, 1999 (their equation 5.24, page 86). The second term under the square root sign is a very different beast - an estimate of the variance of the observed trend. As we point out, our $d1^*$ test is very similar to a standard Student's t-test of differences in means (which involves, in its denominator, the square root of two pooled sample variances).

In testing the statistical significance of differences between the model average trend and a single observed trend, Douglass et al. were wrong to use σ_{SE} as the sole measure of trend uncertainty in their statistical test. Their test assumes that the model trend is uncertain, but that the observed trend is perfectly-known. The observed trend is not a "mean" quantity; it is NOT perfectly-known. Douglass et al. made a demonstrably false assumption.

Bottom line: σ_{SE} is a standard estimate of the uncertainty in a sample mean - which is why we use it to characterize uncertainty in the estimate of the model average trend in equation (12). It is NOT appropriate to use σ_{SE} as the basis for a statistical test between two uncertain quantities. The uncertainty in the estimates of both modeled AND observed trend needs to be explicitly incorporated in the design of any statistical test seeking to compare modeled and observed trends. Douglass et al. incorrectly ignored uncertainties in observed trends.

I hope this answers your first question, and explains why there is no inconsistency between the formulation of our $d1^*$ test in equation (12) and the comments that we made in point #3 [immediately before equation (12)]. As we note in point #3, "While σ_{SE} is an appropriate measure of how well the multi-model mean trend can be estimated from a finite sample of model results, it is not an appropriate measure for deciding whether this trend is consistent with a single observed trend."

We could perhaps have made point #3 a little clearer by inserting "imperfectly-known" before "observed trend". I thought, however, that the uncertainty in the estimate of the observed trend was already made very clear in our point #1 (on page 7, bottom of column 2).

To answer your second question, $d1^*$ gives a reasonably flat line in Figure 5B because the first term under the square root sign in equation (12) (the variance of the model average trend, which has a dependence on

smaller than the second term under the square root sign (the variance of the observed trend, which has no dependence on N). The behaviour of $d1^*$ with synthetic data is therefore dominated by the second term under the square root sign - which is why the black lines in Figure 5B are flat.

In answer to your third question, our Figure 6A provides only one of the components from the denominator of our $d1^*$ test (σ_{SE}). Figure 6A does not show the standard errors in the observed trends at discrete pressure levels. Had we attempted to show the observed standard errors at individual pressure levels, we would have produced a very messy Figure, since Figure 6A shows results from 7 different observational datasets.

We could of course have performed our $d1^*$ test at each discrete pressure level. This would have added another bulky Table to an already lengthy paper. We judged that it was sufficient to perform our $d1^*$ test with the synthetic MSU T2 and T2LT temperature trends calculated from the seven radiosonde datasets and the climate model data. The results of such tests are reported in the final paragraph of Section 7. As we point out, the $d1^*$ test "indicates that the model-average signal trend (for T2LT) is not significantly different (at the 5% level) from the observed signal trends in three of the more recent radiosonde products (RICH, IUK, and RAOBCORE v1.4)." So there is no inconsistency between the formulation of our $d1^*$ test in equation (12) and the results displayed in Figure 6.

Thanks again for your interest in our paper, and my apologies for the delay in replying to your email - I have been on travel (and out of email contact) for the past 10 days.

With best regards,

Ben

Cawley Gavin Dr (CMP) wrote:

>

>

> Dear Prof. Santer,

>

> I think there may be a minor problem with equation (12) in your paper
> "Consistency of modelled and observed temperature trends in the tropical
> troposphere", namely that it includes the standard error of the models
> $1/n_m s_{\langle b_m \rangle}^2$ instead of the standard deviation $s_{\langle b_m \rangle}^2$. Firstly
> the current formulation of (12) seems at odds with objection 3 raised at
> the start of the first column of page 8. Secondly, I can't see how the
> modified test d_1^* gives a flat line in Figure 5B as the test statistic
> is explicitly dependent on the size of the model ensemble n_m . Thirdly,
> the equation seems at odds with the results depicted graphically in
> Figure 6 which would suggest the models are clearly inconsistent at
> higher levels (400-850 hPa) using the confidence interval based on the
> standard error. Lastly, (12) seems at odds with the very lucid
> treatment at RealClimate written by Dr Schmidt.

>

> I congratulate all 17 authors for an excellent contribution that I have
> found most instructive!

>

> I do hope I haven't missed something - sorry to have bothered you if
> this is the case.

> Best regards

>

> Gavin

>

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-3840
FAX: (925) 422-7675
email: santer1@llnl.gov

[REDACTED]

From: mchandler1@gmail.com on behalf of Chandler, Mark A. (GSFC-611.0)[COLUMBIA UNIVERSITY]
Sent: Sunday, December 14, 2008 6:24 PM
To: gs210@columbia.edu
Subject: Re: EdGCM?

I'm trying to think this through - we've had additional requests since it's been taken off line. I want to be careful though because EdGCM is something of an "asset" that I'm not sure I really want to give away. I've invested so much time and funding in this that I need to be realistic about preserving value. We've been very "philanthropic" about it for years with the hope that some government entity would recognize all we do and support it. But, if they won't I may take it in a different direction. Furthermore, in recent years there have been universities and schools that have paid for it and for support - it is somewhat unfair to them to give it away to some and charge others. I can't say I've handle this too well - I should've been more consistent from the beginning, but I was naive about it. Finally, in a sense it has been open source, just as the GCM is, but little has ever happened along those lines. I'll keep you informed. For now, I guess we may rethink our decision to remove it from our website (perhaps putting it back up with a comment about NO support). I'll keep you informed as we go forward.

Bye, Mark

On Fri, Dec 12, 2008 at 1:10 PM, <gs210@columbia.edu> wrote:
Mark,

I hear all of that and obviously I'm sympathetic. But isn't the answer just to put the whole thing out there as an open source project and tap into the expertise of the user base? No promises of support, no dealing with queries - just let the forums and the user base do it?

You aren't paying anything for the server right? Therefore your overhead costs on keeping this running should be close to zero (as long as your time isn't being used). If you were interested, there might some way to have RealClimate help launch this as an open source project perhaps?

Gavin

Quoting Mark Chandler <mchandler@giss.nasa.gov>:

Hi Gavin,

We've been trying for a couple years to find ANY other way to stay alive with this. I think you know I've put a lot of effort into this project.

Sadly, there's just too much discrepancy between demand for EdGCM and lack of funding. The conundrum is that as the EdGCM client list grows so does the need for support and development (to keep up with continuously changing desktop computing needs and to pursue updated GCMs). As you know, soft-money just isn't a growth industry. We tested the idea this summer and fall of charging for training and support but even those dollars (educational institutions can only pay small amounts) aren't anywhere near enough to keep me or my dwindling group afloat. Michael Shopsin is now long gone. Ken is leaving on a cruise to Antarctica and then off to grad school. Linda and I are on life support with regards to paleoclimate funding....so....the EdGCM

situation is a retrenching for now. I'm not giving up on the idea, but it has to have a stronger foundation to build on.

Mark

On Fri, Dec 12, 2008 at 9:26 AM, <gs210@columbia.edu> wrote:

Hi Linda, Mark,

I saw the latest statement on the EdGCM website about the cessation of downloads, isn't there another way here? I realise that funding is hard to come by, but there are other models for software development and support that might be better than this semi-privatisation. Happy to hear your thoughts on this.

Gavin

--

Mark A. Chandler
Columbia University - NASA/GISS
2880 Broadway, New York, NY 10025
NY office (212) 678-5644; cell (608) 445-0166

--

Mark A. Chandler
Columbia University - NASA/GISS
2880 Broadway, New York, NY 10025
NY office (212) 678-5644; cell (608) 445-0166

To: Thomas L Clune <Thomas.L.Clune@nasa.gov>
Subject: [Fwd: [Esmf_info] ESMF Internal Release v4.0.0]
Date: 05/15/09 19:49:32

anything in here we need to worry about as we start the AR5 runs?

gavin

-----Forwarded Message-----

> From: Cecelia DeLuca <cdeluca@ucar.edu>
> To: esmf_info@ucar.edu
> Subject: [Esmf_info] ESMF Internal Release v4.0.0
> Date: 15 May 2009 17:37:19 -0600

> Dear all,

> ESMF internal (beta) release v4.0.0 is now available from the
> ESMF website, <http://www.esmf.ucar.edu>. Click the "Download"
> tab on the website, then "View All Releases" on the left navigation
> bar, and go to the v4.0.0 entry in the table.

> There is a great deal of new work in this release, reflected in the
> lengthy set of release notes! It includes many improvements to the
> ESMF regridding functionality: grid masking, corrections to the
> algorithm for higher order regridding, options for the pole treatment,
> and delivery of an off-line regridding capability that takes in source
> and destination grid netCDF files and produces interpolation weights.
> There are still pieces to finish up for public release - for example,
> the pole treatments are only in the off-line version and the off-line
> version only handles higher order interpolation, not bilinear - so read
> the release notes and known bugs carefully. Other big changes: There
> were major upgrades to Attributes, which now offer better support for
> reading, writing, and copying Attribute hierarchies. User-specified
> methods can be attached to and executed from States, a capability that
> will improve support for data assimilation. And native Windows is a
> supported platform.

> All the fixes and functionality in the 3.1.0rp2 patch were carried
> over into v4.0.0.

> Overall, this release is significantly more robust than the previous
> v3.1.1 beta release. Capabilities that were introduced for the first
> time in v3.1.1 have matured, and this beta release is the candidate
> for a public release in three months. We would advocate waiting for
> the public release to update production systems.

> However, we strongly encourage you to try this release and provide
> feedback. If you do try it, please pay special attention to the two
> release notes that relate to keyword syntax: these both have bolded
> recommendations that describe modifications you may need to make
> to existing user code. See the v4.0.0 table entry at:

> <http://www.esmf.ucar.edu/download/releases.shtml>

> for documentation, supported platforms, release notes, and known bugs.

> As always, write esmf_support@ucar.edu if you have any questions.

> The ESMF team.

>

> Release notes (relative to internal release v3.1.1):

>

- > * Added public C interfaces for select methods of the Mesh class.
- > The prefix for these methods is ESMC. We do not yet have a
- > Reference Manual that describes these interfaces.
- > * Modifications were made to selected GridComp and CplComp methods
- > that enable use of keyword syntax for procedure arguments. This
- > was not permitted before because they did not have explicit
- > interfaces. Using keyword syntax ensures that the compiler will
- > check argument types. *We strongly recommend that users take
- > advantage of this feature.* All of the ESMF calls that take dummy
- > procedure arguments, or call into previously registered user
- > procedures, now provide explicit interfaces: ESMF_GridCompSetVM(),
- > ESMF_GridCompSetServices(), ESMF_GridCompSetEntryPoint(),
- > ESMF_GridCompInitialize(), ESMF_GridCompRun(),
- > ESMF_GridCompFinalize(), ESMF_GridCompReadRestart(),
- > ESMF_GridCompWriteRestart(), ESMF_CplCompSetVM(),
- > ESMF_CplCompSetServices(), ESMF_CplCompSetEntryPoint(),
- > ESMF_CplCompInitialize(), ESMF_CplCompRun(),
- > ESMF_CplCompFinalize(), ESMF_CplCompReadRestart(),
- > ESMF_CplCompWriteRestart().
- > * Those methods that call into a user supplied routine provide two
- > separate (optional) return codes - the standard "rc" return code
- > for the ESMF library, and the "userRc" return code set by the user
- > routine. If keyword syntax is not used, the two return codes can
- > get confused, with dire results. Specifically, using the old
- > interface argument order, without keyword syntax, would lead to
- > incorrect rc association. *Users are strongly encouraged to make
- > appropriate changes to their codes.* See the API changes page for
- > details.
- > * ESMF_GridCompSetServices() and ESMF_CplCompSetServices() now
- > support runtime loading of ESMF components via shared objects.
- > This allows users to maintain separate build systems for
- > individual components in the same application. The newly added
- > ESMF_ArrayRedistSharedObj system test demonstrates this feature.
- > * Changed the behavior of ESMF_GridCompSetServices() and
- > ESMF_CplCompSetServices() to support the MAPL/GEOS-5 Component
- > hierarchy paradigm. For most user code the SetServices change is
- > expected to be completely transparent. All previous versions of
- > ESMF executed the specified SetServices routine in the context of
- > the parent VM. Now, the specified public child routine is executed
- > within the context of the child VM. This means that calls that
- > would affect the child VM must be issued before the child
- > SetServices routine is called. Two new interfaces,
- > ESMF_GridCompSetVM() and ESMF_CplCompSetVM(), have been added that
- > allow child component code to be called and executed from within
- > the context of the parent VM. The benefit of the SetServices
- > change is that the child SetServices routine may now contain calls
- > that require execution from within the context of the child VM,
- > e.g. creation of sub-components.
- > * Added standard ReadRestart and WriteRestart methods to the
- > ESMF_GridComp and ESMF_CplComp interfaces.
- > ~~* The ESMF_GridCompGet() and ESMF_CplCompGet() interfaces now offer~~
- > ~~access to the current method (for example, initialize, run, ...)~~
- > ~~and current phase of a Component.~~
- > * Added "attachable methods" to ESMF States. User supplied methods

> methods cannot be copied or moved between States. For details see
 > the new "Attachable Methods" section in the "Infrastructure:
 > Utilities" part of the ESMF Reference Manual.
 > * The InternalState documentation in the Reference Manual has been
 > reworked to be more applicable to real world situations.
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 > * The ESMF_FieldRegridStore() call now supports source and
 > destination masking. The user can use this masking to tell the
 > regrid to ignore source and/or destination points.
 > * The ESMF_FieldRegridStore() call now has an unmappedDstAction
 > flag. This allows the user to choose if they want unmapped
 > destination points to cause an error or to be ignored.
 > * A number of bug fixes and tweaks have improved the accuracy and
 > stability of ESMF_FieldRegridStore(). This is in particular true
 > of the higher-order interpolation weight generation.
 > * Added a new public method ESMF_FieldGetBounds() to allow a user to
 > query localDe specific data bounds.
 > * Grids now have the capability to store other types of data besides
 > coordinates. This data is referred to as Grid items. A set of new
 > interfaces which mirror the ESMF_GridCoord interfaces has been
 > added to Grid to support this functionality. The Grid currently
 > supports four items and a set of new flags (ESMF_GRIDITEM_MASK,
 > ESMF_GRIDITEM_AREA, ESMF_GRIDITEM_AREAM, ESMF_GRIDITEM_FRAC) has
 > been added to be used in the interface to indicate the item being
 > accessed. The ESMF_GRIDITEM_MASK item is used to store mask
 > information for use by ESMF_FieldRegridStore().
 > * Added support to create arbitrarily distributed Grids using
 > ESMF_GridCreateShapeTile(), ESMF_GridCreate(), and
 > ESMF_GridSetCommitShapeTile(). Other Grid interfaces have been
 > modified as needed. Grid items do not work for arbitrarily
 > distributed Grids.
 > * Changed ESMF_GridGet() interface to get the information defined
 > for the arbitrarily distributed Grid, including arbDim (the
 > dimension in the DistGrid that represents the arbitrarily
 > distributed dimension(s)), localCount (the number of PET-local
 > arbitrarily distributed index locations), and localIndices (a list
 > of PET-local arbitrarily distributed index locations). The names
 > of these arguments are likely to change in future releases.
 > * Added ability to read Attributes from an XML file via new method
 > ESMF_AttributeRead(), which then attaches the Attributes to a
 > Component, Field, or Grid.
 > * XML Attribute file reading is done with the open source third
 > party Xerces C++ library. ESMF must be built against the Xerces
 > library, which requires the setting of additional ESMF environment
 > variables. See the "Third Party Libraries" section in the ESMF
 > User's Guide for details.
 > * Implemented ability to specify time units as 8-byte floating point
 > values in Time Manager methods.
 > * Implemented ESMF standard Attribute packages for Components,
 > State, Field, and Grid.
 > * Added Attributes to ArrayBundle and DistGrid.
 > * Allow Field Attribute hierarchy to connect to Grid Attribute
 > hierarchy.

> FieldBundle, and Field.

- > * Allow ESMF_AttributeCopy() to locally copy Attribute values between Components.
- > * Added ESMF_AttributeUpdate() routine to build consistent Attribute structure and values for objects defined across a VM. One use of this feature is to use it in conjunction with ESMF_AttributeCopy() during the CplComp run method in order to transfer Attributes between GridComps defined on mutually exclusive sets of PETs.
- > * Overloaded ESMF_AttributeRemove() to remove entire Attribute packages, Attributes in an Attribute package, or just single Attributes.
- > * Added the ability to nest Attribute packages inside of each other.
- > * ESMF_StateReconcile() now has a flag to allow optional Attribute reconciliation. The default is currently that Attribute reconciliation is off, due to concerns about performance and robustness.
- > * Added a set of overloaded ESMF_AttributeLinkRemove() interfaces. These detach an object's Attribute hierarchy from that of another object.
- > * Modified the ESMF_Attribute system test to use ESMF_AttributeUpdate(), ESMF_AttributeCopy(), and ESMF_StateReconcile() on both States and Components where appropriate.
- > * The range of Fortran unit numbers used within ESMF can be overridden via the ESMF_Initialize() call by using the new IOUnitLower and IOUnitUpper optional arguments. A new ESMF_IOUnitGet() method to find an unused unit number within the ESMF unit number range is provided as a service to application developers.
- > * ESMF_IOUnitFlush() is provided as a common method to flush Fortran unit numbers. This is intended for use as a service to application developers.
- > * Added ESMF_ConcurrentEnsemble system test demonstrating an ensemble in which different ensemble members, which are distinctly different components, run concurrently on distinct sets of PETs.
- > * Added ESMF_InternalStateEnsemble system test demonstrating how different ensemble members representing the same Component can be written using a single Component object which indexes different elements of an ESMF internal State.
- > * Activated TestHarness for Array and Field testing.
- > * Added support for Windows MinGW/MSYS, using the Intel Fortran and C++ compiler and Microsoft MPI. This was developed and tested under Windows Compute Cluster Server (CCS) 2003.
- > * Standardized 3rd party library support (NETCDF, PNETCDF, LAPACK, XERCES).
- > * An offline file-to-file regridding application has been developed as part of ESMF. This application takes two netcdf grid files and outputs interpolation weights generated using ESMF's higher-order interpolation. This application supports a number of options for handling the pole: no pole, treating the pole as the average of the values surrounding the pole, and an n-point stencil average of the points surrounding the pole. The format of the files makes this application a suitable replacement for SCRIP. For details see the new "File Based Regrid Weight Application" section in the "Infrastructure: Fields and Grids" part of the ESMF Reference Manual.
- > * Furthermore, all the features listed in the release notes for ESMF_3_1_0rp2 apply to this release.

Subject: Re: [Fwd: [Esmf_info] ESMF Internal Release v4.0.0]

Date: 05/18/09 14:32:18

ok, thanks. We'll push ahead and work on this later.

gavin

On Mon, 2009-05-18 at 09:03, Tom Clune wrote:

> Hi Gavin,

>

> I will ask around to some others to see what unspoken items may be of
> concern, but generally speaking I would caution against an upgrade to
> 4.0.0 at this time. I base this recommendation on the assumption
> that stability is your primary concern at this time for AR5. In
> reality, you're not using any of the functionality that has changed,
> so other than exposure of a major bug, I would instead encourage you
> to stay consistent with the version that is working for you.

>

> 4.0.0 probably does represent an opportunity to more consistently
> introduce ESMF within modelE because (a) interfaces should be (almost)
> permanent now and (b) much of the missing functionality that we have
> kludged together in the past should not be robustly available.
> Note that I've not played with any of this, but I have tasted the kool-
> aid. So once AR5 is frozen, I would encourage a project to examine
> what the long term strategy should be with regard to ESMF and possibly
> follow up with an infrastructure project to address any conclusions
> from that examination.

>

> Cheers,

>

> - Tom

>

>

> On May 15, 2009, at 7:49 PM, Gavin Schmidt wrote:

>

> > anything in here we need to worry about as we start the AR5 runs?

> >

> > gavin

> >

> > -----Forwarded Message-----

> >

> >> From: Cecelia DeLuca <cdeluca@ucar.edu>

> >> To: esmf_info@ucar.edu

> >> Subject: [Esmf_info] ESMF Internal Release v4.0.0

> >> Date: 15 May 2009 17:37:19 -0600

> >>

> >>

> >> Dear all,

> >>

> >> ESMF internal (beta) release v4.0.0 is now available from the

> >> ESMF website, <http://www.esmf.ucar.edu>. Click the "Download"

> >> tab on the website, then "View All Releases" on the left navigation

> >> bar, and go to the v4.0.0 entry in the table.

> >>

> >> There is a great deal of new work in this release, reflected in the

> >> lengthy set of release notes! It includes many improvements to the

> >> ESMF regridding functionality: grid masking, corrections to the

> >> and delivery of an off-line regridding capability that takes in
> >> source
> >> and destination grid netCDF files and produces interpolation weights.
> >> There are still pieces to finish up for public release - for example,
> >> the pole treatments are only in the off-line version and the off-line
> >> version only handles higher order interpolation, not bilinear - so
> >> read
> >> the release notes and known bugs carefully. Other big changes:
> >> There
> >> were major upgrades to Attributes, which now offer better support for
> >> reading, writing, and copying Attribute hierarchies. User-specified
> >> methods can be attached to and executed from States, a capability
> >> that
> >> will improve support for data assimilation. And native Windows is a
> >> supported platform.
> >>
> >> All the fixes and functionality in the 3.1.0rp2 patch were carried
> >> over into v4.0.0.
> >>
> >> Overall, this release is significantly more robust than the previous
> >> v3.1.1 beta release. Capabilities that were introduced for the first
> >> time in v3.1.1 have matured, and this beta release is the candidate
> >> for a public release in three months. We would advocate waiting for
> >> the public release to update production systems.
> >>
> >> However, we strongly encourage you to try this release and provide
> >> feedback. If you do try it, please pay special attention to the two
> >> release notes that relate to keyword syntax: these both have bolded
> >> recommendations that describe modifications you may need to make
> >> to existing user code. See the v4.0.0 table entry at:
> >> <http://www.esmf.ucar.edu/download/releases.shtml>
> >> for documentation, supported platforms, release notes, and known
> >> bugs.
> >>
> >> As always, write esmf_support@ucar.edu if you have any questions.
> >>
> >> Cheers,
> >> The ESMF Team.
> >>
> >> Release notes (relative to internal release v3.1.1):
> >>
> >> * Added public C interfaces for select methods of the Mesh class.
> >> The prefix for these methods is ESMC. We do not yet have a
> >> Reference Manual that describes these interfaces.
> >> * Modifications were made to selected GridComp and CplComp methods
> >> that enable use of keyword syntax for procedure arguments. This
> >> was not permitted before because they did not have explicit
> >> interfaces. Using keyword syntax ensures that the compiler will
> >> check argument types. *We strongly recommend that users take
> >> advantage of this feature.* All of the ESMF calls that take dummy
> >> procedure arguments, or call into previously registered user
> >> procedures, now provide explicit interfaces: ESMF_GridCompSetVM(),
> >> ESMF_GridCompSetServices(), ESMF_GridCompSetEntryPoint(),
> >> ESMF_GridCompInitialize(), ESMF_GridCompRun(),
> >> ESMF_GridCompFinalize(), ESMF_GridCompReadRestart(),
> >> ESMF_GridCompWriteRestart(), ESMF_CplCompSetVM(),
> >> ESMF_CplCompSetServices(), ESMF_CplCompSetEntryPoint(),

```

>>> ESMF_CplCompWriteRestart().
>> * Those methods that call into a user supplied routine provide two
>> separate (optional) return codes - the standard "rc" return code
>> for the ESMF library, and the "userRc" return code set by the user
>> routine. If keyword syntax is not used, the two return codes can
>> get confused, with dire results. Specifically, using the old
>> interface argument order, without keyword syntax, would lead to
>> incorrect rc association. *Users are strongly encouraged to make
>> appropriate changes to their codes.* See the API changes page for
>> details.
>> * ESMF_GridCompSetServices() and ESMF_CplCompSetServices() now
>> support runtime loading of ESMF components via shared objects.
>> This allows users to maintain separate build systems for
>> individual components in the same application. The newly added
>> ESMF_ArrayRedistSharedObj system test demonstrates this feature.
>> * Changed the behavior of ESMF_GridCompSetServices() and
>> ESMF_CplCompSetServices() to support the MAPL/GEOS-5 Component
>> hierarchy paradigm. For most user code the SetServices change is
>> expected to be completely transparent. All previous versions of
>> ESMF executed the specified SetServices routine in the context of
>> the parent VM. Now, the specified public child routine is executed
>> within the context of the child VM. This means that calls that
>> would affect the child VM must be issued before the child
>> SetServices routine is called. Two new interfaces,
>> ESMF_GridCompSetVM() and ESMF_CplCompSetVM(), have been added that
>> allow child component code to be called and executed from within
>> the context of the parent VM. The benefit of the SetServices
>> change is that the child SetServices routine may now contain calls
>> that require execution from within the context of the child VM,
>> e.g. creation of sub-components.
>> * Added standard ReadRestart and WriteRestart methods to the
>> ESMF_GridComp and ESMF_CplComp interfaces.
>> * The ESMF_GridCompGet() and ESMF_CplCompGet() interfaces now offer
>> access to the current method (for example, initialize, run, ...)
>> and current phase of a Component.
>> * Added "attachable methods" to ESMF States. User supplied methods
>> can be attached, executed and removed. Currently attached user
>> methods are ignored during ESMF_StateReconcile(), and attached
>> methods cannot be copied or moved between States. For details see
>> the new "Attachable Methods" section in the "Infrastructure:
>> Utilities" part of the ESMF Reference Manual.
>> * The InternalState documentation in the Reference Manual has been
>> reworked to be more applicable to real world situations.
>> * Added support in ESMF_FieldCreate() methods for Fields on
>> arbitrarily distributed Grids. ESMF_FieldRegrid() does not yet
>> work for arbitrarily distributed Grids. Overall, communications
>> using Fields on arbitrarily distributed Grids have not been well
>> tested.
>> * The ESMF_FieldRegridStore() call now supports source and
>> destination masking. The user can use this masking to tell the
>> regrid to ignore source and/or destination points.
>> * The ESMF_FieldRegridStore() call now has an unmappedDstAction
>> flag. This allows the user to choose if they want unmapped
>> destination points to cause an error or to be ignored.
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>> stability of ESMF_FieldRegridStore(). This is in particular true
>> of the higher-order interpolation weight generation.

```

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> >> * Grids now have the capability to store other types of data besides
> >> coordinates. This data is referred to as Grid items. A set of new
> >> interfaces which mirror the ESMF_GridCoord interfaces has been
> >> added to Grid to support this functionality. The Grid currently
> >> supports four items and a set of new flags (ESMF_GRIDITEM_MASK,
> >> ESMF_GRIDITEM_AREA, ESMF_GRIDITEM_AREAM, ESMF_GRIDITEM_FRAC) has
> >> been added to be used in the interface to indicate the item being
> >> accessed. The ESMF_GRIDITEM_MASK item is used to store mask
> >> information for use by ESMF_FieldRegridStore().
> >> * Added support to create arbitrarily distributed Grids using
> >> ESMF_GridCreateShapeTile(), ESMF_GridCreate(), and
> >> ESMF_GridSetCommitShapeTile(). Other Grid interfaces have been
> >> modified as needed. Grid items do not work for arbitrarily
> >> distributed Grids.
> >> * Changed ESMF_GridGet() interface to get the information defined
> >> for the arbitrarily distributed Grid, including arbDim (the
> >> dimension in the DistGrid that represents the arbitrarily
> >> distributed dimension(s)), localCount (the number of PET-local
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> >> * XML Attribute file reading is done with the open source third
> >> party Xerces C++ library. ESMF must be built against the Xerces
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> >> variables. See the "Third Party Libraries" section in the ESMF
> >> User's Guide for details.
> >> * Implemented ability to specify time units as 8-byte floating point
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> >> State, Field, and Grid.
> >> * Added Attributes to ArrayBundle and DistGrid.
> >> * Allow Field Attribute hierarchy to connect to Grid Attribute
> >> hierarchy.
> >> * Allow ESMF_AttributeWrite() to be called from Components, State,
> >> FieldBundle, and Field.
> >> * Allow ESMF_AttributeCopy() to locally copy Attribute values
> >> between Components.
> >> * Added ESMF_AttributeUpdate() routine to build consistent Attribute
> >> structure and values for objects defined across a VM. One use of
> >> this feature is to use it in conjunction with ESMF_AttributeCopy()
> >> during the CplComp run method in order to transfer Attributes
> >> between GridComps defined on mutually exclusive sets of PETs.
> >> * Overloaded ESMF_AttributeRemove() to remove entire Attribute
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> >> * ESMF_StateReconcile() now has a flag to allow optional Attribute
> >> reconciliation. The default is currently that Attribute
> >> reconciliation is off, due to concerns about performance and
> >> robustness.
> >> * Added a set of overloaded ESMF_AttributeLinkRemove() interfaces.
> >> These detach an object's Attribute hierarchy from that of another
> >> object.
> >> * Modified the ESMF_Attribute system test to use

```


> Thomas Clune
> Acting Head of the Software Integration and Visualization Office
> NASA GSFC (610.3)
> 301-286-4635 (W) 240-266-0400 (F)
> <Thomas.L.Clune@nasa.gov>
>
>
>
>
>

To: David Sutherland <dsutherland@whoi.edu>
Subject: Re: oxygen isotope data in recent paper?
Date: 06/02/09 10:55:16

Hi David, thanks. A matlab matrix is fine as long as it is obvious what all the columns are.

Gavin

PS. Guest postings are always welcome!

On Tue, 2009-06-02 at 10:26, David Sutherland wrote:

> Hi Gavin,

>

> It is not online anywhere, but the data from the 2004 cruise along the
> SE Greenland shelf is public now and I'd be happy to send it to you.

> Right now it's in matlab format along with a suite of other variables
> (all in one matrix). Would that work, or do you want individual cast
> files? I am also not the one who processed the isotope data, but can
> try to answer any questions you might have. Cheers,

>

> Dave

>

> ps- I sincerely enjoy the blog and see it as a great service to the
> rest of us scientists sitting on our laurels!

>

> On Jun 1, 2009, at 8:39 PM, Gavin Schmidt wrote:

>

> > Dr. Sutherland,

> >

> > I note that you reported on a substantial amount of oxygen isotope
> > data

> > in sea water in your recent JGR-Oceans paper. Is this available in an
> > online archive anywhere? I would greatly appreciate being able to
> > include it in my global database of seawater isotope values:

> >

> > Schmidt, G.A., G. R. Bigg and E. J. Rohling. 1999. "Global Seawater
> > Oxygen-18 Database". <http://data.giss.nasa.gov/o18data/>

> >

> > Of course, if you know of other sources of such data from your
> > research

> > that I may not have come across, I would be most grateful for any
> > leads.

> >

> > Regards,

> >

> > Gavin

> >

> > --

> > *

> > Gavin Schmidt	NASA/Goddard Institute for Space Studies
> >	2880 Broadway
> > Tel: (212) 678 5627	New York, NY 10025

> > |

> > | gschmidt@giss.nasa.gov <http://www.giss.nasa.gov/~gavin>

> > | *

> >

> >

Subject: Re: oxygen isotope data in recent paper?

Date: 06/02/09 13:52:09

Thanks! It's now uploaded.

Gavin

On Tue, 2009-06-02 at 11:19, David Sutherland wrote:

> Okay- here is the .mat file that contains the data matrix and a cell
> array that tells you what each column is. Note there isn't as much
> Alkalinity data as O₁₈ data (they are NaN'ed out). The only thing
> missing is a time stamp for each value- they were all taken within
> about 5 days of each other, centered around Aug-1-2004. Thanks for
> amassing all this! Cheers,

> Dave

> On Jun 2, 2009, at 7:55 AM, Gavin Schmidt wrote:

> > Hi David, thanks. A matlab matrix is fine as long as it is obvious
> > what
> > all the columns are.

> > Gavin

> > PS. Guest postings are always welcome!

> > On Tue, 2009-06-02 at 10:26, David Sutherland wrote:

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> >> It is not online anywhere, but the data from the 2004 cruise along the
> >> SE Greenland shelf is public now and I'd be happy to send it to you.
> >> Right now it's in matlab format along with a suite of other variables
> >> (all in one matrix). Would that work, or do you want individual cast
> >> files? I am also not the one who processed the isotope data, but can
> >> try to answer any questions you might have. Cheers,

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> >> ps- I sincerely enjoy the blog and see it as a great service to the
> >> rest of us scientists sitting on our laurels!

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> >>> Schmidt, G.A., G. R. Bigg and E. J. Rohling. 1999. "Global Seawater

10. Leonid Polyak <polyak.l@osu.edu>

Subject: Re: d18O data submission

Date: 06/02/09 15:15:33

On Tue, 2009-06-02 at 15:07, Leonid Polyak wrote:

> Thanks Gavin,

>

> No problem with the delay. The reference is fine for now, and we can
> modify it later depending on the publication.

>

> BTW, I now read RealClimate - thanks for your efforts with this.

Don't waste too much time there..... ;)

gavin

> Leonid

>

>

> > Joe, Leonid,

> >

> > I've now uploaded your data into the database (v1.18) - sorry for the
> > delay (many other pressures on my time). I've referenced it as Polyak
> > and Ortiz (2009, pers. communication). Let me know if that needs to
> > change.

> >

> > You can reference this as:

> >

> > Schmidt, G.A., G. R. Bigg and E. J. Rohling. 1999. "Global Seawater
> > Oxygen-18 Database (v1.18)". <http://data.giss.nasa.gov/o18data/>

> >

> > Thanks again for submitting your data,

> >

> > Gavin

> >

> >

> >

> >

> > _____ Èíôîðlàöëÿ îð ESET NOD32 Antivirus, ââðñèÿ áàçÛ ààííÛð ñèãíàðððð àèðóñíà
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> >

> > <http://www.esetnod32.ru>

> >

> >

> >

>

Cc: James.E.Hansen@nasa.gov, Makiko.Sato-1@nasa.gov, klo@giss.nasa.gov, gavin@giss.nasa.gov
Subject: Re: GISTEMP algorithm changes around April 2006
Date: 06/30/09 19:40:51

Dear Mr. Carrera,

Thank you for your interest in our work and for critically looking at our results.

Small changes are to be expected in the "historical" data even if our procedure does not change for the following two reasons:

- The monthly files we use as our basic data always contain late reports (sometimes several years back) and corrections of previously reported data.
- All data, including the new ones, are used in our method to cope with missing data in the whole time series.

Such changes may have an impact comparable to the changes caused by the April 2006 procedural modification.

However, I could verify your findings by reversing the change made in April 2006 using the current data: I got 0.54 C/100 yrs (current) and 0.51 C/100 yrs (old), results very similar to yours.

I also created the maps of the 1880-2005 trends of annual means (see attachment - they show the change for the 125 years, so all has to be multiplied by 100/125 to get changes per century). These maps are hard to tell apart which might be our best justification to describe the change as negligible.

To really solve your mystery, we have to tackle the crucial and surprisingly difficult task of recognizing whether a change in a result is important (i.e. significantly larger than the margin of error) or whether it is insignificant.

Best estimate for the margin of error for the 1880-2005 trend of annual global means is about 0.1 C/century, i.e. all we know is that the trend in that period was about 0.5C/century, most likely somewhere between 0.4 and 0.6C/century. The correction introduced by the ice masking change is less than half the margin of error, hence not significant.

By the way, trends starting after 1950 were much less impacted by that change, since starting around that year Arctic and Antarctic temperature measurements became available. Computation of the 55-year change from 1950-2005 yielded .56252 C and .56254 C respectively (needless to say, the last 3 digits are totally insignificant; that change may be anywhere between 0.54 and 0.58 C, since the estimated margin of error for that period is about 0.02 C - shorter period, much better station coverage).

Using the change as a percentage of the trend (10%) to assess its importance does not work: If the trend had been zero, any change - no matter how small - would become infinitely important if that method were sound (since any change in that case is infinitely bigger than the trend). The only appropriate quantity to use as comparison to assess a

Sincerely,

Reto Ruedy

On Tue, 2009-06-30 at 11:46 +0200, Jaime Olbés Carrera wrote:

>

> Dear Dr. James Hansen,

>

> I am contacting you because your name is listed as the appropriate
> person to contact with, regarding scientific inquiries about the
> GISTEMP analysis, in the GISTEMP page of the Goddard Institute for
> Space Studies. This contact was suggested to me by Gavin Schmidt, at
> Realclimate.org, who seemed unable to clarify the following mystery to
> me and recommended contacting "the GISTEMP people directly showing the
> evidence for what you are claiming", which is what I am doing now.

>

> In the GISTEMP web page of Updates and Analysis, the following change
> in methodology is listed as the only important change between February
> 06, 2006 and June 23, 2006:

>

> April 2006: HadISST ocean temperatures are now used only for regions
> that are identified as ice-free in both the NOAA and HadISST records.
> This change effects a small number of gridboxes in which HadISST has
> sea ice while NOAA has open water. The prior approach damped
> temperature change at these gridboxes because of specification of a
> fixed temperature in sea ice regions. The new approach still yields a
> conservative estimate of surface air temperature change, as surface
> air temperature usually changes markedly when sea ice is replaced by
> open water or vice versa. Because of the small area of these gridboxes
> the effect on global temperature change was negligible

>

> (underline is mine)

>

> I have had access to GISTEMP GLOBAL Land-Ocean Temperature Index data,
> according to the GLB.Ts+dSST.txt file published by GISS, as it existed
> on Feb 06, 2006 and on Jun 23, 2006. By comparing the data provided in
> both versions, I have found very important differences in the linear
> trends that both datasets provide. For the period 1880-2005, the
> warming trend for the Global, January to December Annual Temperatures
> in the February 06, 2006 version was 0.497°C/century, while in the
> June 23, 2006 version it was 0.547 °C/century. This means a change in
> the Global Warming Trend which is bigger than a 10%.

>

> Given that the GISTEMP page mentions that, with the change in
> methodology which took place in April 2006, "the effect on global
> temperature change was negligible"; given that the Global Warming
> Trend for the period 1880-2005 changed by more than a 10% between
> February 06, 2006 and June 23, 2006; and given that no other important
> methodology change is listed in the Updates and Analysis web page of
> GISTEMP between February 2006 and June 2006, I can only conclude that
> either there was, actually, another important change that is not
> listed in the Updates page between those two dates, or the change in
> April 2006 did have an important effect on global temperature change
> and the explanation entry should be corrected.

>

> I would be very grateful if either you or one of your collaborators in

>
> I look forward for your reply.
>
> Yours sincerely,
>
> Jaime Olbés Carrera
> dnx
> [REDACTED]
> [REDACTED]
> Tlf: (+34) [REDACTED]
> Mobile: (+34) [REDACTED]
> jaime.olbes@[REDACTED]
>
> This email is confidential, etc...
>
--

Reto Ruedy <rruedy@giss.nasa.gov>

GIF image attachment (trends.gif)

PS document attachment (trends.ps)

To: gschmidt@giss.nasa.gov <gschmidt@giss.nasa.gov>

Subject: Re: Proofs of JQS 1314

Date: 07/06/09 13:37:46

BP is really rc and 1950 so we use it only for rc, for others it is just ka or whatever
Chris

Sent from my iPhone

On 6 Jul 2009, at 18:35, "Gavin Schmidt" <gschmidt@giss.nasa.gov> wrote:

>
>
> Just a quick question before I send you the proofs. I was unaware that
> 'BP' is exclusively used for radiocarbon dates, and as far as I can
> tell
> this is not standard across the literature. However, if this is a
> house
> style, then I can adapt the text accordingly, but what is the house
> style
> for absolute dates in the past?

> gavin

> *-----*
> | Gavin Schmidt NASA/Goddard Institute for Space Studies |
> | 2880 Broadway |
> | Tel: (212) 678 5627 New York, NY 10025 |
> | |
> | gschmidt@giss.nasa.gov http://www.giss.nasa.gov/~gavin |
> *-----*

> On Mon, 6 Jul 2009, John Wiley & Sons Ltd. wrote:

>> WILEY
>> Publishers Since 1807
>> THE ATRIUM, SOUTHERN GATE, CHICHESTER, WEST SUSSEX PO19 8SQ
>> Journal: Journal of Quaternary Science
>> Dear Author,
>> Please find attached the proofs of your article together with a cover
>> page which includes important instructions. Please read these
>> instructions carefully.
>> Please swiftly return your proofs, with corrections, to avoid
>> unnecessary delay in the publication of your article.
>> Please note that this is the only chance you have to make
>> corrections to
>> the article, once your article is published online, further
>> corrections
>> cannot be accepted.
>>
>> Barbara Grunwell
>> Content Editor
>> John Wiley & Sons, Ltd.
>> The Atrium, Southern Gate, Chichester, West Sussex, PO19 8SQ U.K.
>>
>> Proof corrections should be returned via e-mail to
>> JQSproofs@wiley.com

>>
>> Looking forward to hearing from you,
>>
>> Kind regards,
>> Barbara Grunwell
>> Content Editor
>> www.interscience.wiley.com/journal/JQS
>> John Wiley & Sons
>> Wiley Bicentennial: Knowledge for Generations
>> 1807-2007
>>
>>
>

To: gschmidt@giss.nasa.gov

Subject: Per our conversation today on a possible climate change presentation at USDOT

Date: 07/14/09 16:27:38

Hi, Dr. Schmidt. My name is Pete Chipman, I spoke with you earlier today about a possible speaking engagement at the U.S. Department of Transportation.

I'm forwarding the description of you that I sent to our USDOT Climate Change Center coordinator. If I'm misrepresenting you in any part, please let me know. I'll be talking to you soon.

Much appreciated,

Pete Chipman

Federal Transit Administration

Office of Budget and Policy

(202) 366-1637

>>Hi, Joanna.

I can't recommend Gavin Schmidt of NASA Goddard Institute for Space Studies highly enough as a guest speaker for the Center. In addition to being a climate change scientist at NASA, he moderates the blog "Real Climate" at www.realclimate.org and is heavily involved with the IPCC and their published reports. Gavin is regularly featured in climate change debates, and I can think of no other lecturer on global warming in terms of his involvement with the current state of the science, and his ability to communicate it to those not in the field. He can also speak to our strategic concerns within the transportation sector, and possibly show us broader implications we have not addressed. I've spoke to him previously, and he has indicated a willingness to travel to DOT on the train from N.Y. if we requested his presence at the Department.

If you're receptive to this idea, I'd be happy to put together a rough outline for a presentation of his and/or see about his availability. I think he would be an excellent follow-up to Dr. MacCracken's presentation and views. You may want to reserve a larger room, though.

Thanks.<<

[REDACTED]

From: Schmidt, Gavin A. (GISS-6110)
Sent: Monday, March 09, 2009 1:40 PM
To: gs210@columbia.edu
Subject: [Fwd: RE: Colloquium request]
Attachments: Colloquium_Info_Form.pdf

-----Forwarded Message-----

> From: Kevin Tritz <ktritz@pppl.gov>
> To: gschmidt@giss.nasa.gov
> Subject: RE: Colloquium request
> Date: 05 Mar 2009 13:42:17 -0500

> Hi Gavin,

> I think what might be interesting for our group is a discussion about
> the models that you use, and some of the physics behind them. For
> example, how do you determine confidence level, and uncertainties in
> your predictions? How do you model the CO2 cycle and other forcings
> and feedbacks. How do you validate the models using present day climate data and/or
> paleoclimate measurements?

> Also, maybe a comment or two about the various attempts to attribute
> warming to natural cycles (e.g. sunspots, cosmic rays, etc...) and
> how/whether these can or can't explain particular observations. What
> successes have climate models had, and what have they under- or
> over-predicted? Will there be any dramatic near-term predicted effects
> that will be good tests for the present models?

> I think the level of technical detail should be somewhat higher than a
> general audience, but certainly not at the level one would use for a
> group of climate change experts. Also, feel free to save a slide or
> two to advertise your new book!

> I've attached a PDF for you to fill out and send (fax) back which will
> be used to post the information for the colloquium, we'll make sure
> that a copy is sent over to GFDL as well so interested people can come
> over. If you send the PDF back as an email attachment, please note
> that the secretarial contact is actually Lisa Gloudon (fax number is the same as on the
> PDF):

> lgloudon@pppl.gov

> You may choose a time for the talk between 1:30-4pm, whenever fits
> your schedule the best. A convenient method to arrive at the lab is a
> taxi cab from the Princeton Junction train station, though if you've
> been to GFDL often, you probably know your way around the area pretty
> well. Also, if your schedule permits, we would be pleased to have you
> take a tour of our facility and/or join us for dinner.

> Thank you,
> Kevin

> -----Original Message-----

> From: Gavin Schmidt [mailto:gschmidt@giss.nasa.gov]

> Sent: Thursday, March 05, 2009 6:54 AM

> To: Kevin Tritz

> Subject: RE: Colloquium request

>

>

> Ok. What kind of presentation would be most suitable? I have a
> standard climate change 101 talk that I use for groups that are
> interested but not familiar with the details, talks specifically on
> what climate models are good for, technical talks on aspects of
> paleo-climate and the relevance to future changes, or a purely image
> based talk based on my new book which is for the general public.

>

> Presumably you have many talks from people at GFDL so perhaps the more
> technical stuff is most relevant? (or do I assume too much?)

>

> gavin

>

> *-----*
> | Gavin Schmidt NASA/Goddard Institute for Space Studies |
> | 2880 Broadway |
> | Tel: (212) 678 5627 New York, NY 10025 |
> | |
> | gschmidt@giss.nasa.gov http://www.giss.nasa.gov/~gavin |
> *-----*

>

> On Wed, 4 Mar 2009, Kevin Tritz wrote:

>

> > Gavin,

> >

> > April 10th would be fine. We were thinking about having the talk
> > sometime mid-afternoon 2-3pm, and will settle on an exact time soon,
> > after we check for other possible conflicts.

> >

> > PPPL can pick up your travel expenses, and provide a tour and dinner
> > afterwards if your schedule allows. I will contact you shortly with
> > additional information.

> >

> > Thank you,

> > Kevin

> >

> >

> > -----Original Message-----

> > From: Gavin Schmidt [mailto:gschmidt@giss.nasa.gov]

> > Sent: Wed 3/4/2009 4:28 PM

> > To: Kevin Tritz

> > Subject: Re: Colloquium request

> >

> > April 8 is not available. Though Fri Apr 10 would work. You might
> > try Drew Shindell or Ron Miller for availability on the wednesday.

> >

> > gavin

> >

> > On Tue, 2009-03-03 at 13:11, Kevin Tritz wrote:

> >> Greetings,

> >>

> >>
> >>
> >> I am an avid reader of realclimate.org, and I was wondering if you
> >> or any of your colleagues would be interested in giving a
> >> colloquium on climate change at the Princeton Plasma Physics
> >> Laboratory, in Princeton, NJ on April 8th (or anytime that week if
> >> Wednesday is not convenient).
> >>
> >>
> >>
> >> Thank you,
> >>
> >> Dr. Kevin Tritz
> >>
> > --
> > *-----
> > -*
> > | Gavin Schmidt NASA/Goddard Institute for Space Studies |
> > | 2880 Broadway |
> > | Tel: (212) 678 5627 New York, NY 10025 |
> > | |
> > | gschmidt@giss.nasa.gov http://www.giss.nasa.gov/~gavin |
> > *-----
> > -*
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>

[REDACTED]

From: Kevin Tritz [ktritz@pppl.gov]
Sent: Monday, March 16, 2009 9:42 AM
To: gs210@columbia.edu
Subject: RE: [Fwd: RE: Colloquium request]
Attachments: UFNVA BLANK FORM-Red.doc

Gavin,

Sounds good, we look forward to your visit. Also, since you're not a US citizen, could you fill out the attached form and return it to Lisa?

Thanks,
Kevin

-----Original Message-----

From: gs210@columbia.edu [mailto:gs210@columbia.edu]
Sent: Mon 3/9/2009 1:56 PM
To: Kevin Tritz
Cc: Lisa Gloudon
Subject: [Fwd: RE: Colloquium request]

Kevin, here is the form filled in.

Let's say 3pm for the talk, and I'll be happy to go out for dinner.

Gavin

> Hi Gavin,
>
> I think what might be interesting for our group is a discussion about
> the models that you use, and some of the physics behind them. For
> example, how
do
> you determine confidence level, and uncertainties in your predictions?
> How
do
> you model the CO2 cycle and other forcings and feedbacks. How do you
validate
> the models using present day climate data and/or paleoclimate measurements?
> Also, maybe a comment or two about the various attempts to attribute
warming
> to natural cycles (e.g. sunspots, cosmic rays, etc...) and how/whether
these
> can or can't explain particular observations. What successes have
> climate models had, and what have they under- or over-predicted? Will
> there be any dramatic near-term predicted effects that will be good
> tests for the
present
> models?
>
> I think the level of technical detail should be somewhat higher than a
> general audience, but certainly not at the level one would use for a
> group

of
> climate change experts. Also, feel free to save a slide or two to
> advertise your new book!
>
> I've attached a PDF for you to fill out and send (fax) back which will
> be used to post the information for the colloquium, we'll make sure
> that a
copy
> is sent over to GFDL as well so interested people can come over. If
> you
send
> the PDF back as an email attachment, please note that the secretarial
contact
> is actually Lisa Gloudon (fax number is the same as on the PDF):
> lgloudon@pppl.gov
>
> You may choose a time for the talk between 1:30-4pm, whenever fits
> your schedule the best. A convenient method to arrive at the lab is a
> taxi cab from the Princeton Junction train station, though if you've
> been to GFDL often, you probably know your way around the area pretty
> well. Also, if
your
> schedule permits, we would be pleased to have you take a tour of our
facility
> and/or join us for dinner.
>
> Thank you,
> Kevin
>
>
>
>
>
> -----Original Message-----
> From: Gavin Schmidt [mailto:gschmidt@giss.nasa.gov] Sent: Thursday,
> March 05, 2009 6:54 AM
> To: Kevin Tritz
> Subject: RE: Colloquium request
>
>
> Ok. What kind of presentation would be most suitable? I have a
> standard climate change 101 talk that I use for groups that are
> interested but not familiar with the details, talks specifically on
> what climate models are good for, technical talks on aspects of
> paleo-climate and the relevance to future changes, or a purely image
> based talk based on my new book which is for the general public.
>
> Presumably you have many talks from people at GFDL so perhaps the more
> technical stuff is most relevant? (or do I assume too much?)
>
> gavin
>
> *-----*> | Gavin Schmidt NASA/Goddard Institute for Space Studies |
> | 2880 Broadway |
> | Tel: (212) 678 5627 New York, NY 10025 |

> | gschmidt@giss.nasa.gov http://www.giss.nasa.gov/~gavin |

```

> *-----*
>
> On Wed, 4 Mar 2009, Kevin Tritz wrote:
>
> > Gavin,
> >
> > April 10th would be fine. We were thinking about having the talk
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> > after we check for other possible conflicts.
> >
> > PPPL can pick up your travel expenses, and provide a tour and dinner
> > afterwards if your schedule allows. I will contact you shortly with
> > additional information.
> >
> > Thank you,
> > Kevin
> >
> > -----Original Message-----
> > From: Gavin Schmidt [mailto:gschmidt@giss.nasa.gov]
> > Sent: Wed 3/4/2009 4:28 PM
> > To: Kevin Tritz
> > Subject: Re: Colloquium request
> >
> > April 8 is not available. Though Fri Apr 10 would work. You might
> > try Drew Shindell or Ron Miller for availability on the wednesday.
> >
> > gavin
> >
> > On Tue, 2009-03-03 at 13:11, Kevin Tritz wrote:
> >> Greetings,
> >>
> >>
> >> I am an avid reader of realclimate.org, and I was wondering if you
> >> or any of your colleagues would be interested in giving a
> >> colloquium on climate change at the Princeton Plasma Physics
> >> Laboratory, in Princeton, NJ on April 8th (or anytime that week if
> >> Wednesday is not convenient).
> >>
> >>
> >> Thank you,
> >>
> >> Dr. Kevin Tritz
> >>
> > -- >
> *-----*
> > | Gavin Schmidt          NASA/Goddard Institute for Space Studies |
> > |                        2880 Broadway                        |
> > | Tel: (212) 678 5627    New York, NY 10025                    |
> > | gschmidt@giss.nasa.gov http://www.giss.nasa.gov/~gavin       |
> > *-----*

```

> >
> >
> >
> >
>

----- End forwarded message -----

Clear

Colloquium Information Form

The following information will appear in our announcement (without editing):

*Your Full Name: _____

*Your Title: _____

*Your Affiliation: _____

*Title of Your Colloquium: _____

Date of Colloquium: _____

Are you eligible for the honorarium? ☐ Yes or ☐ No (If Yes, Social Security # Needed.): _____

*Country of Citizenship: _____

*Country, City and State of Birth: _____

*Abstract of your Talk: *(This information will appear in our announcement.)*

Brief Biography: *(Information for your host.)*

*Contact information:

Business or Home Address: _____

Business or Home Phone, Fax, and E-mail: _____

Audio-Visual equipment you need us to supply *(check all that apply)*:

☐ PC laptop

☐ Mac laptop

☐ Digital projector

☐ 35-mm slide projector

☐ Overhead projector

☐ Videotape or DVD player

☐ Lantern (2" x 3") slide projector

☐ Film projector

☐ Audio tape player

☐ Other (specify): _____

Please return completed form to:

Cynthia Murphy

Princeton Plasma Physics Laboratory

Office of the Deputy Director

P.O. Box 451, MS-37 B384

Princeton, NJ 08543

Tel: 609 243 2484 ♦ Fax: 609 243 2749 ♦ Email: cmurphy@pppl.gov

*Must include this information

UNCLASSIFIED FOREIGN NATIONAL VISIT/ASSIGNMENT FORM
PRINCETON PLASMA PHYSICS LABORATORY

(Page 1 of 2)

*Denotes Required Information

NAME OF VISITOR/ASSIGNEE

*First Name: _____ *Middle: _____ *Last: _____

FORM DETERMINATION INFORMATION

*Facility to be Visited: PPPL

Is this an off-site meeting? ☐ Yes ☒ No

*Select the Security Area Type at the Facility (choose highest area type in case multiple areas are to be visited):

☐ Non-Security Area ☒ Property Protection Area ☐ Limited Area ☐ Exclusion Area
☐ MAA ☐ Protected Area ☐ SCIF

*Country of Employer: _____

*Will sensitive subjects be discussed? ☐ Yes ☒ No

*Is this an IAP-66 (DS-2019) assignment? ☐ Yes ☒ No

*Does the Host have a clearance? ☐ Yes ☒ No

*Type of Request (check one): ☒ Visit ☐ Assignment ☐ Extension of an Assignment ☐ High Level Protocol Visit

BIOGRAPHICAL INFORMATION

*Gender: ☐ Female ☐ Male

Is visitor currently in the U.S.? ☐ Yes ☐ No

*Permanent Resident Alien: ☐ Yes ☐ No

Green Card exp. Date (mm/dd/yy): _____ S.S.# _____

*Country of Citizenship: _____

*Date of Birth (mm/dd/yy): _____

*Country of Birth: _____

*City of Birth: _____

Aliases (optional): _____

EMPLOYER INFORMATION

Affiliation or Company Info:

*Institution or Company Name: _____

Phone Number: _____

Street (1): _____

Fax Number: _____

Street (2): _____

E-mail Address: _____

City: _____

State: _____

Zip Code: _____

*Country of Employer: _____

Title of Position and Duties: _____

VISA INFORMATION

PASSPORT INFORMATION

*Visa or Green Card Number: _____

*Passport Number: _____

*Visa Type: _____

*Country of Issue: _____

*Exp. Date (mm/dd/yy): _____

*Exp. Date (mm/dd/yy): _____

PLACE OF WORK (if different from Employer)

Company Name: _____

Phone Number: _____

Street (1): _____

Fax Number: _____

Street (2): _____

E-mail Address: _____

City: _____

State: _____

Zip Code: _____

*Country: _____

Title of Position and Duties: _____

Interpreter Needed? ☐ Yes ☐ No Business Type conducted by Employer: _____

Educational Background: _____

Field of Research: _____

Accompanying Family Information: _____

Additional Biographical Information: _____

Current U.S. Address: _____

City: _____

Street (1): _____

State: _____

Street (2): _____

Zip Code: _____

Permanent Address: _____

City: _____

Street (1): _____

State: _____

Street (2): _____

Zip Code: _____

Remarks: _____

(continued on Page 2)

UNCLASSIFIED FOREIGN NATIONAL VISIT/ASSIGNMENT FORM
PRINCETON PLASMA PHYSICS LABORATORY
(Page 2 of 2)

VISIT/ASSIGNMENT SPECIFIC INFORMATION

Type of Request: Comes from Form Determination

*Off Site Meeting? ☐ Yes ☒ No

*Is this a High Level Protocol Visit? ☐ Yes ☒ No

*Will Sensitive Subjects be discussed? ☐ Yes ☒ No

HOST INFORMATION

*Host's First Name: _____ Middle: _____ *Last: _____

*Host's Citizenship: _____ *Phone: _____

Does Host have a clearance? ☐ Yes ☐ No

VISIT INFORMATION

*Desired Start Date (mm/dd/yy): _____ *Desired End Date (mm/dd/yy): _____

Subject(s): _____

International Agreement Code: _____

HDE Code: _____

*Justification of Visit/Assignment, including specific activities or involvement: _____

Purpose of Visit: _____

Remarks/Comments (or additional information that did not fit above): _____

Official DOE/PPPL Exchange Agreement Visit? Yes _____ No _____

Visit/Assignment Supported By: _____
(Home Institution, other)

Amount of Financial Support: _____
(Visitors on full salary from U.S. institutions need only so state)

Source of Support: _____
(e.g., DOE contract, etc.)

Expenses to be Paid by PPPL:

Travel:	Yes _____	No _____
Lodging:	Yes _____	No _____
Subsistence:	Yes _____	No _____
Lodging Location/Telephone Number: _____		

Cost Center to be Charged: _____ CC Manager's Signature: _____

Does Visitor Carry His/Her Own Medical Insurance? Yes _____ No _____

Division Head/Date _____ Host/Date _____

Department Head/Date _____ Deputy Director (if required) _____

Send the completed form to: D. Stevenson (Site Protection Division, MS-01)
Send copies to: B. Sobel (Director's Office, MS-37)
Travel Office, MS-11



Site Access Notification Form Completed (check)
Note – this is available via the web at <http://www-local.pppl.gov/SiteAccess.html>
and is required to be completed before visitor arrives.

Was Visit/Assignment Fulfilled? Yes _____ No _____

Actual Start Date: _____ Actual Completion Date: _____

Subject: RE: Request to have you speak at the US Department of Transportation's Climate Change Center

Date: 09/11/09 13:09:39

Hi Peter,

Thanks for your note. It's probably worth re-iterating that I am not a policy expert, nor an energy expert and so growth rates of renewables and/or the performance of Cash-for-Clunkers is a little out of my domain. However, we are starting to be in a position to address the multi-emission consequences of sector-by-sector changes in emission profiles (both for climate, air pollution and public health). I'd be happy to discuss some of the issues associated with those assessments and give some of the preliminary results our group has come up with.

As for very specific experiments tied to transportation, that could certainly be the basis for some future collaborations but for now you'll have to make do with what we've done already.

To get an idea of what that is, these papers are probably useful:

http://pubs.giss.nasa.gov/abstracts/2009/Unger_etal_1.html

http://pubs.giss.nasa.gov/abstracts/2008/Shindell_etal_3.html

gavin

On Fri, 2009-09-11 at 12:48, Peter.Chipman@dot.gov wrote:

> Hi, Dr. Schmidt. I'm about to give you a call. Before I do, I had a little more on what we could have you discuss at the Department.

>

> In the last e-mail I sent, I had a bullet on transportation's greenhouse gas wedge. The bullet was getting at the true impact is of the transportation greenhouse gas wedge (e.g., DOE's and EPA's analysis) compared to our current understanding/perception (it may be the same, less, or broader than we realize).

>

> Below is an example of what we could look at along these lines

>

> I've been following closely the markets for solar, wind, hybrid vehicles, plug-in hybrid vehicles, and electric vehicles the past two years. Of note is the cost per watt of solar and wind, which according to some companies' claims, has already reached or bettered the cost of coal per watt in some designs. Projections for solar show its industry-wide cost may potentially reach parity with coal by 2016, having significant implications for the percentage of new power-plants that have no carbon emissions.

>

> Total wind power capacity is now estimated to be 121 gigawatts--equivalent to about 121 nuclear power facilities. By 2015, it is projected that wind power capacity will reach 425 gigawatts with a growth rate just shy of 20% a year between 2009 and 2015. This means that by 2015, wind may surpass nuclear power in terms of global energy production and could be supplying the world with about 7-10 percent of its electricity.

>

> At the same time, electric vehicles are rapidly developing and will soon become a viable choice for drivers much like hybrids are now. As recent as a year ago, I had conversations with people at EPA and DOT that were of the opinion that even if this technology was coming on line sooner than previously anticipated, fleet turnover would be a major issue. Well, with just \$3 billion in incentives from the Cash for Clunkers program, we very quickly turned over 680,000 vehicles at just about a 10 mpg average improvement per vehicle. What would happen if we did the same program with electric vehicles several years from now and put some real money into it?

> A convergence of a lower-carbon electrical grid and electric vehicles is coming sooner

> This discussion above sets the stage for something you may be able to help us with. If the transportation sector represents 28 to 33% of greenhouse gas emissions (I believe it's about 28% if life-cycle analysis is not included, 33% if it is), could we model what the impact would be if the transportation sector became a zero-carbon source of emissions by 2050? (I'm not sure if all non-carbon greenhouse gases would be eliminated at the same time if you eliminated carbon-based fuels and went to wind, solar, geothermal, wave energy, hydro, etc.). Could you paint us various pictures of the world in 2050 including: transportation emissions BAU; what we currently see as "best case" scenario for the transportation sector in 2050; and a new best case scenario for the transportation sector in which the carbon contribution from transportation is negligible by mid-century? Depending on the degree to which the projected impacts of climate change are reduced, this could be a catalyzing piece of information for the Department, particularly with a new Departmental strategic plan, and a USDOT Climate Change Center strategic plan in development.

>
> If so, we could expand the Department's (and departments responsible for other sectors) view of what scenarios we should be planning for and working towards. Possibly we could stir ourselves out of the inertia we have short of pep-talk/concerned speeches. So, a new "best case" scenario could be transportation is a zero or near-zero carbon-emission sector by 2050 due to the transport being powered by electricity coming from non-carbon sources such as solar, wind, etc. It's not to say that this viewed as the reality for 2050, but rather that the potential should not be excluded in the delta between BAU and the optimum scenario for the transportation sector--it should be the top end of the range rather than what may be a presently constricted view of the optimum. This should not be an effort to "grasp" for a better outcome, but rather recognize that one could reasonably exist and that we should include it in our analysis and scenario-based planning.

>
> I also wonder how this would impact the more general scenarios in the IPCC reports?

>
> Anyways, Ill talk to you soon.

>
> Thank you,

>
> Pete Chipman
> Federal Transit Administration
> (202) 366-1637

>
>
>
>
> -----Original Message-----

> From: Gavin Schmidt [mailto:gschmidt@giss.nasa.gov]
> Sent: Wed 9/2/2009 2:19 PM
> To: Chipman, Peter (FTA)
> Subject: RE: Request to have you speak at the US Department of Transportation's Climate Change Center

>
>
> mon 7, so let's call tuesday or wed.

>
> gavin

>
> *-----*
> | Gavin Schmidt | NASA/Goddard Institute for Space Studies |
> | | 2880 Broadway |
> | Tel: (212) 678 5627 | New York, NY 10025 |
> | |
> | gschmidt@giss.nasa.gov | http://www.giss.nasa.gov/~gavin |

> On Wed, 2 Sep 2009 Peter.Chipman@dot.gov wrote:

>

> > Thank you, Gavin.

> >

> > Enjoy your vacation (is that Monday the 7th or 14th that you're back?)

> >

> >

> >

> > From: Gavin Schmidt [mailto:gschmidt@giss.nasa.gov]

> > Sent: Wed 9/2/2009 1:47 PM

> > To: Chipman, Peter (FTA)

> > Subject: Re: Request to have you speak at the US Department of Transportation's Climate Change Center

> >

> >

> >

> >

> > Hi Pete, I'm on vacation until next monday - but I'd be happy to further

> > discuss this on the phone after I get back.

> >

> > Thanks!

> >

> > Gavin

> >

> > *-----*

> > Gavin Schmidt	NASA/Goddard Institute for Space Studies
> >	2880 Broadway
> > Tel: (212) 678 5627	New York, NY 10025
> >	
> > gschmidt@giss.nasa.gov	http://www.giss.nasa.gov/~gavin

> > *-----*

> >

> > On Wed, 2 Sep 2009 Peter.Chipman@dot.gov wrote:

> >

> >> Hi, Dr. Schmidt.

> >>

> >>

> >>

> >> This is Pete Chipman of the Federal Transit Administration within the

> >> U.S. Department of Transportation, and a member of the USDOT Climate

> >> Change Center.

> >>

> >>

> >>

> >> In mid-July, I approached you about speaking at the U.S. Department of

> >> Transportation on climate change. Belatedly, I would like to approach

> >> you again with a framework for your engagement I've had in mind for

> >> sometime (and not coincidentally, the development of it after our phone

> >> conversation is partly what delayed my follow-up with you).

> >>

> >>

> >>

> >> Before I get into detail as to what we would like to have you engage in,

> >> it is necessary for me to clear up an error on my part. When I spoke

> >> with you, I mentioned that you would be a follow-up of contrasting

> >> opinion to Dr. Mike MacCracken of the Climate Institute. At the time, I

> >> did not realize that I was confusing Dr. MacCracken with Kyle Isakower

> >> Department. My apologies if you were wondering why I thought your
> >> lecture would be in contrast to Dr. MacCracken's, as obviously you are
> >> both of the same scientific aim of objectivity and rigorousness of
> >> testing theory. I think I may have even chuckled a bit on the phone
> >> thinking how much we needed a contrasting opinion to Isakower
> >> (mistakenly thinking he was MacCracken at that time). I very much
> >> enjoyed Dr. MacCracken's lecture, subsequent to Isakower's, and talked
> >> to him at length after the session he presented.

> >>
> >>
> >>
> >> Now that that's out of the way...

> >>
> >>
> >> It's my opinion that people like you who have the technical background
> >> and skill to clarify climate science through concise and attainable
> >> conceptualization can be instrumental in "jump-starting" Federal
> >> institutions. They can do so by helping calibrate those organizations
> >> on more effective courses towards coordinated climate change solutions.
> >> Specifically, strengthening DOT's scientific consciousness and ability
> >> to evolve organizationally with that science could dramatically enhance
> >> our incremental improvements over Business As Usual impacts within our
> >> sector. In our case, understanding more the magnitude of what needs to
> >> be accomplished within the transportation sector to mitigate and adapt
> >> to climate change is imperative (the sector is representative of 28 to
> >> 33% of greenhouse gas emissions depending on how you count it). Just as
> >> important is the ability to be organizationally adaptive to variables
> >> that play out in the coming years, allowing us to have more capacity to
> >> respond quickly to a changing planet, and even reduce the inertia common
> >> in government that can impair proactiveness in favor of a reactive
> >> approach.

> >>
> >>
> >>
> >> We are currently reviewing our U.S. DOT Climate Change Center strategic
> >> plan, and some of the following considerations would be helpful to cover
> >> during your visit:

> >>
> >> --The delta between scientific consensus that feeds into the IPCC and
> >> the politically acceptable reports that come out of the IPCC. This may
> >> put the numbers in some of our reports (e.g., our sea-level rise studies
> >> related to transportation infrastructure) in a new context. This would
> >> also hopefully lead to a greater capacity to stay in tune with cutting
> >> edge data, vs. interim or 7-year reports.

> >>
> >>
> >>
> >> --Your perspective on what the "true" impact is of the transportation
> >> greenhouse gas wedge (e.g., DOE's and EPA's analysis) compared to our
> >> current understanding/perception (it may be the same, less, or broader
> >> than we realize).

> >>

> >>
> >> --A better understanding of how to navigate science presented in the
> >> media (is it out of context, is it over- or under-hyped, is it a rehash

> >>
> >>
> >> --A discussion of some of the Climate Change Centers key documents
> >> (provided to you in advance), such as our report to Congress on the
> >> impact transportation has on climate change and potential solutions.
> >> Discussion of our draft CCC strategic plan may be useful as well.
> >>
> >>
> >>
> >> --Any insight you may have into Federal institutions that have made
> >> dramatic shifts in their approach to climate change, and what change in
> >> organizational culture facilitated that.
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> >>
> >> --Anything that you feel we may be overlooking.
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> >>
> >> I know your time is limited, but I do think that your role could be very
> >> impactful if, for a day at least, your usual speaking engagement goes
> >> into these areas as well as a discussion as to what you think we can do
> >> as an organization to be more effective in leading our sector towards
> >> solutions in climate change.
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> >>
> >> After the discussion, and as we develop the new strategic plan for the
> >> CCC, it would be good opportunity to revisit where we are and discuss how
> >> we are evolving. This set of dialogue could be used as a model for
> >> future engagements you may have with other Federal departments if the
> >> outcomes from our experience are valuable and substantive.
> >> Obviously the Department of Energy, EPA, NASA, etc. are already planning
> >> and are advanced in their strategic and operational efforts. However,
> >> other areas like housing, trade, public lands, etc., may or may not have
> >> this advanced set of plans, or they may be "home grown" with
> >> intermittent outside analysis. You could really provide a honing of
> >> these types of Departmental efforts so that more and more we are all
> >> speaking, engaging, and working on the same page. If this becomes
> >> common practice, it could lead to a multi-faceted network that can
> >> evolve and work strategically to develop the architectures we need to
> >> mitigate and adapt to the warming planet as effectively as possible (at
> >> least the U.S.'s part of the pie).
> >>
> >>
> >>
> >> I look forward to working with you on this matter and seeing what
> >> progress we can make. The USDOT Climate Change Center has some very
> >> talented and committed individuals. Your insights will not go unused.
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> >>
> >> Possibly we could discuss these issues by phone Thursday or Friday, or
> >> next week..

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> >>
> >>> Pete Chipman
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> >>
> >>> Federal Transit Administration
> >>
> >>>
> >>
> >>> Office of Budget and Policy
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> >>>
> >>
> >>> (202) 366-1637
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> >>
> >>>>> Hi, Joanna.
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> >>
> >>> I can't recommend Gavin Schmidt of NASA Goddard Institute for Space
> >>
> >>> Studies highly enough as a guest speaker for the Center. In addition
> >>
> >>> to being a climate change scientist at NASA, he moderates the blog
> >>
> >>> "Real Climate" at www.realclimate.org and is heavily involved with the
> >>
> >>> IPCC and their published reports. Gavin is regularly featured in
> >>
> >>> climate change debates, and I can think of no other lecturer on
> >>
> >>> global warming in terms of his involvement with the current state of
> >>
> >>> the science, and his ability to communicate it to those not in the
> >>
> >>> field. He can also speak to our strategic concerns within the
> >>
> >>> transportation sector, and possibly show us broader implications we

> >> have not addressed. I've spoke to him previously, and he has
> >>
> >>> indicated a willingness to travel to DOT on the train from N.Y. if we
> >>
> >>> requested his presence at the Department.
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> >>
> >>> If you're receptive to this idea, I'd be happy to put together a rough
> >>
> >>> outline for a presentation of his and/or see about his availability.
> >>
> >>> I think he would be an excellent follow-up to Dr. MacCracken's
> >>
> >>> presentation and views. You may want to reserve a larger room,
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> >>> though.
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> >>
> >>> Thanks.<<
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>

Cc: Michael Way <michael.j.way@>, Erik Noble <enoble@giss.nasa.gov>

Subject: Re: idea for wednesday talks

Date: 09/16/09 21:01:51

it's being discussed here:

<http://www.realclimate.org/index.php/archives/2009/09/communicating-science-not-just-talking>

and there were a bunch of reviews on ScienceBlogs earlier this year. I'd just watch it if I was you.

gavin

On Wed, 2009-09-16 at 20:35, bastiaan van Diedenhoven wrote:

> Thanks Gavin. We'll keep this in mind. I'm curious to know what the
> global warming comedy is about!

>

> Bastiaan

>

> On Wed, Sep 16, 2009 at 6:17 PM, Gavin Schmidt

> <gschmidt@giss.nasa.gov> wrote:

> I occasionally get DVDs of vaguely relevant documentaries that
> might be

> of some interest. For instance, "Sizzle: A global warming
> comedy" and "A

> Sea Change" (about overfishing and ocean acidification). They
> aren't

> that long (80 mins or so), so a lunchtime screening could be
> doable if

> you don't have anyone lined up.

>

> Gavin

>

>

>

>

> --

>

> -----

> Dr. Bastiaan van Diedenhoven

> Columbia University Center for Climate Systems Research /

> NASA Goddard Institute for Space Studies

> 2880 Broadway, Room 316C

> New York, NY 10025

> Phone: (212) 678 5512

> Fax: (212) 678 5552

> Email: bvandiedenhoven@giss.nasa.gov

> URL: <http://www.columbia.edu/~bv2154/>

> -----

>

TO: Munro, Neil <nmunro@nationaljournal.com>

Subject: Re: from Munro

Date: 09/25/09 17:04:52

pinatubo impacts:

Hansen, J., A. Lacis, R. Ruedy, and Mki. Sato, 1992: Potential climate impact of Mount Pinatubo eruption. Geophys. Res. Lett., 19, 215-218, doi:10.1029/91GL02788.

<http://pubs.giss.nasa.gov/cgi-bin/abstract.cgi?id=ha00800v>

predictions from the 1980's

Hansen, J., I. Fung, A. Lacis, D. Rind, Lebedeff, R. Ruedy, G. Russell, and P. Stone, 1988: Global climate changes as forecast by Goddard Institute for Space Studies three-dimensional model. J. Geophys. Res., 93, 9341-9364, doi:10.1029/88JD00231.

<http://pubs.giss.nasa.gov/cgi-bin/abstract.cgi?id=ha02700w>

A discussion:

<http://www.realclimate.org/index.php/archives/2007/05/hansens-1988-projections/>

Attached: A draft article on models and policy

the model simulation archive at PCMDI

http://www-pcmdi.llnl.gov/ipcc/about_ipcc.php

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*-----*
| Gavin Schmidt          NASA/Goddard Institute for Space Studies |
|                        2880 Broadway                             |
| Tel: (212) 678 5627    New York, NY 10025                       |
|                        |                                         |
| gschmidt@giss.nasa.gov  http://www.giss.nasa.gov/~gavin          |
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PDF document attachment (PWOct09schmidtREV.pdf)